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MIND

A QUARTERLY REVIEW
OF
PSYCHOLOGY AND PHILOSOPHY

EDITED BY
PROF. GILBERT RYLE
WITH THE CO-OPERATION OF PROF. SIR F. C. BARTLETT AND PROF. C. D. BROAD

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MIND

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INCREASE IN SUBSCRIPTION

The subscription to Mind Association, and the annual payment for "MIND" to the publishers or to booksellers, will be raised as from 1 January 1960 from one guinea to twenty-five shillings. There will be no increase in the U.S. Subscription ($4.00).

It is regretted that this increase comes so soon after the previous one notified in 1957. This is due to the increased costs resulting from the award after the recent printers' dispute. Costs have also risen owing to increased postal charges in 1957 and printers' cost of living bonuses since 1957.

The accounts published in this issue show a balance of £67 but the accounts include no payment to the Contributors' Fund, which normally requires £50 to £60 a year. There is therefore no margin at present.

Subscribers who have already renewed their subscriptions at the old rate for 1960 will be allowed to continue at this rate until their subscription expires.

TREASURER'S REPORT

The Association has 1,090 subscribing members and twenty-nine life members. The accounts for the year ending 31 March 1959, are as shown below.

Receipts	£	s.	d.	Expenditure	£	s.	d.
From Publishers . .	1104	0	1	Aberdeen Printers .	2175	11	7
,, Subscriptions .	1181	4	8	Oxford Printers . .	3	15	0
Dividends and Interest	73	15	0	Editor's Salary . .	160	0	0
Sale of Review Books .	152	2	8	Editor's Expenses .	75	11	4
				Treasurer's Expenses .	25	6	0
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				Profit on year's working	67	16	2
	£2511	2	5		£2511	2	5

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ASSETS	£	s.	d.
Balance (General) .	937	6	7
,, (Contributors' Fund) .	171	16	9
U.S. Subscriptions due	265	16	3
Other Subscriptions due	52	10	0
Investments at cost .	1801	11	9
	3229	1	4
Less sundry creditors .	105	17	8
	£3123	3	8

J. D. MABBOTT,
Hon. Treasurer.

I.—PROOF AND THE THEOREM PROVED

BY ALICE AMBROSE

IN his *Remarks on the Foundations of Mathematics* Wittgenstein writes: "One would like to say that the understanding of a mathematical proposition is not guaranteed by its verbal form, as is the case with most non-mathematical propositions."[1] The point of this comment, or at least an important point, comes out in the account he gives of mathematical proof, in particular of the connection between the proof of a statement and the meaning of the statement. This account has difficulties, which Wittgenstein has himself pointed out, but the resolution of which remains obscure. I shall not pretend here to present how he intended to circumvent these difficulties, as this would involve me in doubtful interpretation and extension of what he has said. What I wish to do in this paper is to elaborate the difficulties inherent in his description of proof and to indicate where it is, and where it is not, possible to escape them.

I begin with his account of the connection between a proof and the statement proved. As is well known, he frequently asserted that "if you want to know what a mathematical statement means, look at its proof". The concluding expression of a sequence of expressions constituting the statement of a proof derives its sense from the proof, and it does not have that sense apart from the proof. This means that what one understands at the end of a proof is different from what one understood by the same sentence at the beginning. Concretely, if "There are no odd perfect numbers" is proved, one's understanding of the sentence expressing it will differ from one's present understanding. Similarly for the unproved statements "Every even number is the sum of two primes", "There are infinitely many pairs of primes of the form p and $p + 2$". Wittgenstein's account of the connection between the proof of a statement and its meaning is supported by his claim that in advance of proof we do not know what it is like for the statement to be true, since we cannot describe what it is like to establish its truth. A problem in mathematics for which as yet there is no solution "is like the problem set by the king in the fairy tale who told the princess to come neither naked nor dressed, and she came wearing fish net. He did not really know what he wanted her to do but when she came thus he was forced to accept it. His order was of the form:

---

[1] P. 147.

Do something which I shall be inclined to call neither naked nor dressed. It is the same with the mathematical problem: Do something which one will be inclined to accept as a solution; though one doesn't know what it will be like."[1] Being unable to describe a possible solution—since this would be to give it—he took as a reason for saying we do not know what it is like for the statement in question to be true, and this in turn as a reason for saying we do not know its meaning, and that proof gives it its meaning. To put his point alternatively, an expression in the interrogative does not have the sense it has once the answer is provided; proof " provides it with content ".[2]

The following series of questions discussed by Wittgenstein[3] illustrates his thesis: Is there a 6 in the first thousand places in the development of $\frac{1}{7}$? Is there a 2 in the infinite development? Are there two 2's ? Are there three consecutive 7's in the expansion of π ? In the case of the first two questions mere continued division is an adequate method of giving the answer: that there is no 6 in the first thousand places, that there is a 2 at the third place and hence in the infinite development. But suppose that in the process of calculating one discovered periodicity. The question concerning a 6, and its answer, according to him now has a new interpretation, namely, that there is no 6 in the period. The interpretation is provided by the new method of answering it, a calculation embodied in the formula Remainder = dividend (which is not merely a calculation yielding several repetitions of the same group of numbers). This method also shows that there cannot be two 2's in the development. But in the case of π there is no method for showing either that there must be three 7's or that there cannot be; there is as yet no operation comparable to the division which yields a 2 at the third place in the development of $\frac{1}{7}$ nor any rule comparable to Remainder = dividend which precludes a pair of 2's.

Because the rule for expanding π determines what occurs at *each* place in the series it might appear that the ground for the decision was already there, and that we could decide between the existence of three 7's and the existence of no such triple had we the omniscience to survey an infinite expansion. But omniscience cannot surmount the logically impossible.[4] Nor could it settle the question by its grasp of the rule of expansion: " Even God can determine something mathematical only by mathematics. Even for him the mere rule of expansion cannot decide anything that it does not decide for us. We might put it like this: if the rule for the expansion has been given us, a *calculation* can tell us that there is a '2' at the fifth place. Could God have known this, without the calculation, purely from the rule of expansion? I want to say: No."[1] If, then, omniscience fails both to survey the infinite expansion and to see what the rule must or must not yield, and hence fails to settle the question regarding π, the situation is not to be described as one in which there is something to know which we do not know. Rather, there is as yet no calculus for it.[2] The ground for the decision has yet to be invented.[3] Were this question to become decidable it would change its status: "For a connexion is made then, which formerly *was not there*."[4] Before the mathematics has been invented which will provide a decision this question does not make sense—just as "in an arithmetic in which one does not count further than 5 the question what $4 + 3$ makes doesn't yet make sense" . . . though "the problem may very well exist of giving this question a sense".[5]

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[1] From lecture notes taken in 1934-35. [2] *Ibid.* [3] *Ibid.*

[4] This I have argued in " Finitism and ' The Limits of Empiricism ' ", MIND, vol. xlvi, no. 183.

There is something curiously opaque about Wittgenstein's assertions concerning the connection between proof of a proposition *p*, sense of the sentence "*p*", and understanding "*p*". The difficulties inherent in his account are apparent, and he has himself mentioned them, while leaving their resolution obscure. I shall now detail them, so that it will be clear what difficulties must be got round and what clarification is required. First, if a question lacks content because nothing in the body of mathematics developed so far is capable of yielding the answer to it, then apparently the question must already be answered in order to be called a question.[6] To *what* question does the proof provide an answer? Wittgenstein admits that "to say it is the proof which gives sense to the question is absurd because it misuses the word 'question'".[7] "Answer" is in the same case. If an interrogative expression puts no question, then there is nothing to which anything one demonstrates is an answer. Further, if "a mathematical proposition has no sense before being proved true or false",[8] then what has the mathematician proved? Proof neither provides an answer to the question set nor establishes that a *given* proposition has a truth-value. The proposition the mathematician attempts to prove, if there is one at all, is not identical with the proposition proved, which is to say that he does not prove what he sets out to prove. For if proof *gives* sense to an expression, then prior to proof the expression did not have *that* sense. That sense which exists only as the result of proof, cannot be described as " what was to be proved ". No proof should conclude with the words " Quod erat demonstrandum ". This means that what one understands at the end of a proof is different from what one understood by the same sentence in the beginning. Either one understands nothing at all before knowing the proof or one understands something different after knowing it. Further, if the expression concluding the sequence of sentences expressing the proof gets its sense from the proof it would seem to follow that there could not be two proofs of the same proposition. Wittgenstein was aware of these paradoxical consequences : " . . . ought I to say that the same sense can only have *one* proof ? Or that when a proof is found the sense alters ? Of course some people would . . . say ' Then the proof of a proposition cannot ever be found, for, if it has been found, it is no longer the proof of *this* proposition ' ".[1] His rejoinder is : " But to say this is so far to say nothing at all." [2] " Of course it would be nonsense to say that *one* proposition cannot have two proofs—for we do say just that." [3]

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[1] *Remarks on the Foundations of Mathematics*, p. 185.

[2] Lecture notes, 1934-35.

[3] *Remarks on the Foundations of Mathematics*, p. 138.

[4] *Ibid.*

[5] *Ibid.* p. 140.

[6] Lecture notes, 1934-35.

[7] *Ibid.*

[8] *Ibid.*

This is not in the least helpful. The puzzle remains. Perhaps it will be useful to see that whatever point Wittgenstein had in mind cannot be expressed by the words " proof gives '*p*' its sense ". Suppose we ask " proof of what ? ". Is it intelligible to be told : Proof *of p* provides " *p* " with sense ? Clearly it is nonsense to say that proof that a proposition expressed by " *p* " has a certain truth-value makes " *p* " express that proposition. It will be observed that the language used to assert a connection between proof of *p* and sense of " *p* " is in what one might call the " mixed mode " of speech. Language referring to a proposition turns out to be language which should refer to a sentence (or *vice versa*). That is, " proposition " or " statement " is used ambiguously, now to mean " sentence ", now to mean " sense of a sentence ". " *p* is necessary in language L ", " a statement *s* is meaningful if it can be verified or falsified ", are similar examples of the same mixed use of language. The first expression can be satisfactorily replaced by " '*p*' expresses a necessary proposition in L ", but the second when rendered unambiguous clearly fails to express a criterion of meaningfulness : " *s* is meaningful " requires *s* to be an expression, " *s* can be verified " requires that

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[1] *Remarks on the Foundations of Mathematics*, p. 164.

[2] *Ibid.*

[3] *Ibid.* p. 92.

s be what the expression means. The situation is similar with "proof of a statement gives it sense": "statement" is ambiguous. And when rendered unambiguous the claim becomes: an expression acquires meaning when one establishes the truth-value of what it already means.

I see no way to interpret Wittgenstein's assertion so as to make it a sensible description of a deduction. However, I think a reconstruction of it can be given which correctly describes certain pieces of mathematical reasoning, misleadingly called proofs, as giving meaning to an expression. Further, it is possible to remove the paradox in his claim that what one understands by the terminal sentence of the sequence of sentences expressing the proof is different from what one understands before it takes its place in such a sequence. I should like to consider this latter point first. To this end it will be enlightening to note certain facts about sentences expressing necessary propositions, *i.e.* the kind of propositions figuring within mathematics.

Consider the sentence: "It is logically impossible for a heptagon to be constructed with straight edge and compass." The fact that this sentence expresses a necessary proposition implies that the phrase "heptagon constructed with straight edge and compass" does not serve to describe any figure. If the phrase did have a descriptive use, then it would describe something which would falsify a necessary proposition. This fact about the way language is used prevents the sentence from saying anything falsifiable. That is, the sentence expresses a necessary proposition *because* its empirical verbal correlate, "'heptagon constructed with straight edge and compass' has no descriptive use", expresses what is true.[1] This is not to say that it *asserts* that its verbal correlate says what is true, else the necessary proposition expressed by "No heptagon is constructible with straight edge and compass" would be the same as the empirical proposition about the use in our language of the phrase "heptagon constructed with straight edge and compass". But neither is it about a sort of figure, heptagon constructible with ruler and compass, the existence of which it asserts to be impossible. If it were it would be precisely similar to the proposition that it is impossible for iron to be melted in boiling water. "Iron melting in boiling water" denotes the kind of occurrence which would falsify the proposition; it *describes* what is declared to be impossible, a conceivable state of nature. If then such a sentence as " It is impossible for a heptagon to be constructed with straight edge and compass " is not about a kind of thing which it declares to be impossible, what do we understand by it ? What do we know ? The sentence does not *express* any fact about the use of words. Yet all that is left for one to know in understanding it is such a fact. There is no inconsistency between these two claims about the sentence, for what is known in understanding it is not the same as what it expresses.[1]

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[1] For the detail of this description of sentences expressing necessary propositions, see M. Lazerowitz, *The Structure of Metaphysics*, pp. 265-271.

This account can be brought to bear on the question as to what the mathematician understands when he is in the position of having to ask, " Does ' *p* ' express something true or something false ? ", *e.g.* " Does ' There are no odd perfect numbers ' express a truth or a falsity ? ". To raise this question about a sentence which does not terminate any sequence of sentences expressing a demonstration is to betray ignorance of whether its descriptive part, *e.g.* " odd perfect number ", has a use. And in this circumstance he can justly be said not to understand the sentence. Prior to finding out whether its descriptive part has a use he does not know what the proposition is which is expressed by it, whether an impossible one or a necessary one. To know the truth-value of what is expressed by a sentence terminating the statement of proof is to know *what* is expressed. This seems to me good reason (although no such reason may have been in Wittgenstein's mind) for saying that one understands after proof, *i.e.* once one has a sequence of statements constituting proof, something different from what one understood beforehand.

It may seem paradoxical to assert that, properly speaking, we do not understand a sentence for an *a priori* proposition whose truth-value we do not know, whereas we do understand such a sentence as " It is impossible to construct a heptagon with straight edge and compass ". To assert this is to emphasize the difference between the two cases, in particular, to emphasize what we do *not* know in the case of " There are no odd perfect numbers ". What proposition this combination of words asserts we do not know, nor therefore whether " odd perfect number " has or has not a use. In the case of " heptagon constructible with straight edge and compass ", proof shows that it is to be excluded from our notation, as denoting no figure, and we can hardly claim to have understood by it the same concept as before this was shown, or for that matter, that we understood this *combination* of words at all. We *can* claim to understand "There are no odd perfect numbers" if understanding it comes to nothing more than knowing the combination of words to be grammatical and understanding the individual words. To claim this is to emphasize the similarity between the two cases. It does not matter whether we describe ourselves as understanding a sentence in both sorts of cases so long as we note the difference which proof effects in what we know. For to signalize this difference by asserting that only after proof do we understand a sentence in effect merely restricts the scope of the word "understand". The claim seems paradoxical because it seems to deny a fact rather than to bear on our use of language.

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[1] *The Structure of Metaphysics*, p. 270, where this distinction is discussed.

Let us now consider Wittgenstein's assertion that proof gives sense to a mathematical statement. As has been seen, the expression "proof gives sense to 'p'" is nonsense if "proof" is understood in the sense of a straightforward deduction. But there are procedures in mathematics which are called proof and which have the air of being demonstrations but whose work is actually to give meaning to an expression. These may have been in Wittgenstein's mind when he said this. Consider Cantor's proof by means of his diagonal procedure that the set of real numbers is non-denumerable. Of this Wittgenstein says: It "does not shew us an irrational number different from all in the system, but gives sense to the mathematical proposition that the number so-and-so is different from all those of the system. . . . Cantor gives a sense to the expression 'expansion which is different from all the expansions of the system' by proposing that an expansion should be so called when it can be proved that it is diagonally different from the expansions in a system. . . . These considerations may lead us to say that $2^{\aleph_0} > \aleph_0$. That is to say: we can *make* the considerations lead us to that. Or: we can say *this* and give *this* as our reason."[1] The last three sentences are particularly important. They give one an idea as to what he had in mind in saying "One would like to say: the proof changes the grammar of our language, changes our concepts. It makes new connexions. (It does not establish that they are there; they do not exist until it makes them.)"[2] "A proof induces us to make a certain decision, namely that of accepting a particular concept-formation."[3]

Let us now consider several simple examples. It is usual to find in textbooks an *argument* for equating a° with 1. To quote from N. J. Lennes's *College Algebra*: "Since $a^m \div a^m = a^m/a^m = 1$ and also $a^m \div a^m = a^{m-m} = a^\circ$, it follows that $a^\circ = 1$ for all values of a."[1] The comment made by Lennes upon this bit of reasoning is significant: "Hence we will adopt the following definition: $a^\circ = 1$."[1] This comment makes explicit what the role of the "proof" is: to justify the introduction of the expression "$a^\circ = 1$" and to give it meaning. For the definition given of an exponent, as the number of times an expression is used as factor, *i.e.* is multiplied by itself, provides no meaning for the expression "a°". That is, the definition does not make a° a member of the power series a^n, for it does not define "a^n" for $n = 0$. Without the reasoning, "a°" and "$a^\circ = 1$" would lack sense. What is set out as a proof that the *proposition* $a^\circ = 1$ is *true* is in fact a reason for giving use to the *expression* "$a^\circ = 1$". Similarly, the "demonstration" that any number different from zero cannot be divided by zero and that zero divided by zero may represent any number whatever provides the reason for excluding the expression "$a/0$". Most often a justification for introducing or excluding an expression in mathematics is not presented as such, but rather is presented as a piece of reasoning showing something to be true or false. Thus, $a^\circ = 1$ is "proved" to be true, division by zero is "proved" to be impossible. A decision about language, a linguistic innovation, is often concealed by the form of words in which it is introduced, *i.e.* by the language of demonstration of truth or falsity. The idiom natural to the doing of mathematics is that in which expressions are used rather than mentioned, and it is often not as clear as in the examples above that the reasoning adduced is a justification for a decision with respect to an expression rather than a proof of the truth or falsity of what is expressed by it. For example, it is said that in order to ensure that linear equations and equations of higher degree always have solutions the domain of numbers was extended by the addition of negative integers, rationals, reals, and complex numbers. This way of putting the matter conceals the fact that extension of the domain of numbers consisted in introducing new symbols and a use for them which assured meaning to the expressions "$a + x = b,\ b < a$", "$ax = b$, a not a factor of b", "$x^2 = a$, a not an integral square", "$x^2 = -a$". The latter expressions and the expressions "$-(a - b)$", "b/a", "$\sqrt{a}$", "$\sqrt{-a}$" simultaneously acquire meaning. To say that prior to the introduction of negative integers $a + x = b$ did not always have a solution suggests that the expression "$a + x = b$" was meaningful even when $b < a$; actually it was not.

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[1] *Remarks on the Foundations of Mathematics*, p. 58.

[2] *Ibid.* p. 79.

[3] *Ibid.* p. 122.

It is instructive to compare the so-called proof that $a^\circ = 1$ and the proof of the Fundamental Theorem of Euclid: If a and b are any two positive integers there exist integers q and r, $q \geq 0$, $0 \leq r < b$, such that $a = qb + r$. Here there are two cases to consider: (1) a is a multiple of b, (2) a is not a multiple of b, where (2′) $a > b$, (2″) $a < b$. In case (1) the theorem is verified, r being 0. In case (2′) a must lie between two consecutive multiples of b: qb and $(q + 1)b$; whence there is an integer r, $0 < r < b$, such that $a = qb + r$. In case (2″), $q = 0$ and $r = a$. Q.E.D. The deduction here moves between strictly delimited concepts—positive integer, the relations greater than, less than, equal to, the operations of addition and multiplication. No concept-word undergoes an extension of meaning in the course of the proof. The proof merely makes explicit what the initial concepts imply: it is analogous to a calculation, say to that which shows a 6 in the product 13 × 13, where once the rules are accepted one is not free to operate arbitrarily. In such cases it is appropriate to speak of discovery of a mathematical fact.

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[1] P. 40.

How is it with the so-called proof that $a^\circ = 1$? Wittgenstein's claim that "a proof is not found but constructed",[1] that "a proof induces us to make a certain decision, namely that of accepting a particular concept-formation",[2] fits this case. Keeping in mind that when a sentence expresses a necessary proposition a certain expression has no use, consider the equations "$a^m \div a^m = a^{m-m} = a^\circ = 1$". To accept "$a^m \div a^m = a^\circ$" as expressing a logical necessity is to accept a reason for refusing a use to "$a^\circ \neq 1$". And the decision to accept it mathematicians might not have made, just as Descartes refused to describe $x^2 = -1$ as having a solution, or $x^3 + x^2 + x + 1 = 0$ as having three roots. No such freedom of decision exists in the course of the proof of the Fundamental Theorem of Euclid. In equating a° and 1, under the guise of a proof explicating the concept of exponent, an additional rule for the use of "a^n" is argued for. And the acceptance of the reason is expressed in the non-verbal idiom as "proof that $a^\circ = 1$". This it appears to me illustrates how Wittgenstein's claim that proof gives sense to an expression is to be construed. Mathematics in such a case can appropriately be described as invention.

The point can be brought out by means of a distinction made by M. Lazerowitz[3] in describing two different activities of philosophers which go by the name of "analysis", and which have their analogue in mathematical deductions: what he calls actual analyses and "conversion" analyses. As has been argued, a necessary proposition, in which an analysis eventuates, sets out in the non-verbal idiom the criteria for the usage of an expression. Actual analysis, as contrasted with " conversion " analysis, merely makes explicit those features in virtue of which an expression is in accordance with established usage applied (this, of course, without entailing any factual statement about usage). But a conversion analysis calls attention to verbal analogies in order to justify extending, or contracting, the use of an expression, or eliminating an expression altogether. This it usually does under the guise of mere explication, as though the usage it in fact proposes is a matter of record.[1]

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[1] Lecture notes, 1934-35.

[2] *Remarks on the Foundations of Mathematics*, p. 122.

[3] "Moore and Philosophical Analysis", *Philosophy*, July 1958.

Consider the necessary truth that $x^3 + x^2 + x + 1 = 0$ has three roots, $-1, +\sqrt{-1}, -\sqrt{-1}$. Had a different decision been made about this and similar equations—had the decision been to say it had but one root—then we should not say it is true that there are three. And that mathematicians made the decision they did rested on accepting a linguistic innovation: on giving " $\sqrt{-1}$ ", or " i ", a use. The term " i^2 " was *defined* as -1, and the symbol " $a + bi$ " (where " a " and " b " denote real numbers) was so defined as to obey the commutative, associative, and distributive laws of addition and multiplication holding for real numbers. That is, the expressions " $(a + bi) + (c + di)$ ", " $(a + bi) \times (c + di)$ ", etc., were given a use, a use so similar to " $a + c$ " and " $a \times c$ " that the invention of complex number symbols is described in the non-verbal form of speech as an *extension* of the field of real numbers. With this, the simple expressions " $x^2 = -1$ " and " ' $x^2 = -1$ ' has two roots " have meaning. Further, *every* quadratic equation $ax^2 + bx + c = 0$, the roots of which are $-b \pm \sqrt{(b^2 - 4ac)} / 2a$, could be said to have roots both when $b^2 - 4ac \geq 0$ and when it is < 0. " Every quadratic equation has two roots " *came* to express a necessity; that is to say, that it expressed a necessity did not derive from already accepted rules for the use of words, but from an extension of the use of " root ". Similarly, " $a^\circ = 1$, for $n = 0$ " does not record an analytic fact backed by the definition of " exponent ". If it did, mathematiciams would not have been at liberty to accept the definition and deny that for $n = 0$, $a^\circ = 1$. Nor, had complex numbers fallen under the concept of " root " analytically, would they have been at liberty to decide whether the definition of " root " as " value of x satisfying $x^2 = -1$ " covered $\pm\sqrt{-1}$.

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[1] " Moore and Philosophical Analysis ", *Philosophy*, July 1958, for an illustration from philosophy: his study of " Causation is nothing more than invariable conjunction ".

The point I wish to make is that in cases such as those cited, and doubtless in many cases of theorems connected intimately with a notational innovation, what is presented in the form of demonstrative reasoning for a conclusion is actually a justification for a linguistic alteration. This fact renders harmless the puzzling consequence of Wittgenstein's assertion that proof gives sense to a statement, namely, that the mathematician does not prove what he sets out to prove. For in these cases he does not set out to establish the truth-value of a given proposition. To suppose he does is to be misled by the language of demonstration he uses. Where a proof proceeds by an actual analysis the language of demonstration does what it appears to do: it states the reasoning for a proposition's truth, whereupon we know that some expression, *e.g.* " integer not representable uniquely as a product of primes ", has no use. But a conversion analysis, which is also presented in the form of proof, does something else. When Kummer is described as having invented ideal numbers which he proved were uniquely factorable into primes, the description conceals what his reasoning in fact did. It put forward an additional rule for the use of " number uniquely representable as a product of primes " and induced others to accept it. Similarly, when Hamilton is said to have discovered quaternions for which he " showed " that $i \times j \neq j \times i$, it would be less misleading to say that he justified a new use of " × ". Agreement by mathematicians that the reasoning has justified a certain usage is usually signalized by their saying that such-and-such has been proved. Sometimes the use is either not accepted or not agreed on. Weyl's requirement that an existence proof construct that which is said to exist covertly recommends in the non-verbal mode of speech that " There exists . . ." be used only when a method of construction can be given. The opposing positions over the " validity " of non-constructive proofs of existence theorems is in fact a dispute over the latitude to be given " There exists . . .".

To sum up, I have placed on the assertion " proof gives sense to a mathematical statement " a construction which makes it unobjectionable as a claim about certain pieces of reasoning misleadingly called proof. My explanation of the sense in which we can be said to understand by the terminal expression of a sequence of expressions stating a proof something different from what we understood by it originally holds *a fortiori* of these so-called proofs.

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II.—THE UNCONSCIOUS

By Ilham Dilman

What those who talked of "Freud's discovery of the unconscious" had in mind is a group of innovations which the "founder of psycho-analysis" brought to bear on the study of the human mind. It is a set of inter-related innovations which grew over a stretch of time as Freud struggled to deepen his insight, tried to bring relief to his patients, and attempted to express his growing insight and systematise his findings. Let us begin by reminding ourselves of some of the more important of these :

1. That not all thoughts, beliefs, motives, intentions, and emotions are such that their subjects are aware of them. Thus there are cases where we can say, "He thinks that so-and-so and yet he does not know it", "He is envious of so-and-so and yet he is not aware of it", "He has such-and-such a wish and yet he does not recognise it".

2. That people can be responsible for things without recognising their responsibility, that they can pursue policies without realising that they do so—unconscious agency. For instance, a person may act in such-and-such a way or refrain from doing so-and-so without recognising that he has had any part in bringing about the ensuing state of affairs. What he fails to recognise responsibility for may range from what he does to others through his influence on them to what he does in and for himself. He may, for instance, be responsible for not recognising this or that about himself. A detailed account of how this failure comes about and all that goes into fixing it in its final form will bring to light the place here for the notion of responsibility.

3. That all human behaviour[1] is in part unconsciously determined—generality of unconscious determination.

4. That a man's words and thoughts may be treated as on a par with the rest of his behaviour ; and that the evidential significance of his assent to or dissent from statements about himself and of his conscious conviction about their truth or falsity is of the same kind as the one which the rest of his talk and beliefs has. Freud made extensive use of this in the technique he developed for investigating his patients' minds and conveying to

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[1] The notion of human behaviour is used in this article in a way which is general and noncommittal. When more precise concepts become necessary terms such as 'action', 'thought', 'verbal report', 'association', and 'symptom' will be used.

them the insight which their associations gave him. It is worth stressing here that the extent to which psycho-analytic technique suggests thoughts to be susceptible of interpretation regardless of their truth-value is somewhat alien to the common sense attitude to thought which is inclined to dismiss any further questions about a belief held by a particular person once the question of its truth is settled positively.

5. That many of every person's actions, thoughts and feelings are *partly* in accordance with a mentality which he does not recognise, and which is, moreover, so different in kind from the one he is familiar with in himself that he would find it difficult even to entertain the idea of owning it if this is suggested to him —primary processes. Freud's insight into this mentality—highly emotive, out of touch with external reality, and unmodified by any of the learning which goes to change man from his infancy onward—naturally gave a characteristic twist into that part of his explanatory account of human behaviour which he judged important to emphasise. It is mainly this which makes many of Freud's accounts so sharply at variance with common sense psychology.

No doubt the items which I have thus separated and descriptively indicated above are closely interrelated and by no means uniform in character. In what has been talked of as " Freud's discovery of the unconscious " logical, empirical-interpretative, technical and theoretical innovations merge into a whole. There are many different problems connected with each of the above items, and it may be worth noting here that we need to be clear about the notion of unconscious determination in order to grasp properly why Freud insisted on " mental determinism ". However, I shall deal here with some of the most important philosophical problems which arise in connection with only the first item of the above list, *i.e.* the notion of unconscious mental objects. But before entering the main topic of this article there is one point which I think is worth making at the outset of a treatment of Freud's key concept.

Philosophers have drawn our attention of late to the fact that there are many different sorts of question which can be asked about human behaviour (for instance, Professor Ryle, Professor Stephen Toulmin, G. E. M. Anscombe, R. S. Peters); and some have claimed that in developing his theory of the unconscious Freud ignored this fact. The view has also been advanced that Freud developed his theory in order to provide answers to only one sort of question about human behaviour. Thus the former view claims that Freud implicitly denied that there are questions of more than one sort which can be asked about human behaviour, while the latter allows that he acknowledged this while denying that he intended to answer questions of more than one sort. Both these views, I submit, are mistaken.

The falsity of the view that Freud intended his explanations referring to unconscious processes to apply only to a restricted class of cases (*e.g.* " where the purposive rule-following model breaks down or is inappropriate "—XIV, p. 11), and that all such explanations provide answers which are appropriate to only one sort of question about man (*e.g.* where the appropriate answer is a causal explanation—XIV, p. 11) will come to light in this article. To claim that Freud intended his explanations to enlighten us only about a restricted class of cases, such as abnormal behaviour or trance-like activities, where it may seem that some of the familiar sorts of question about human beings are inappropriate or have no answer, is to misunderstand him profoundly. Such a view also ignores Freud's doctrine of the generality of unconscious determination—see item 3 above.

With respect to the former view I would like to insist that Freud's doctrine of the generality of unconscious determination is logically independent of any of his theories which claim to explain *all* human behaviour in terms of, *e.g.* attempting to derive gratification and to avoid pain. The doctrine that all human behaviour is in part unconsciously determined does not prejudge the issue about the sorts of question which can be asked about any piece of human behaviour, nor about the number or kind of aims in terms of which human behaviour is to be explained. One reason why Dr. Peters thinks otherwise (*viz.* that it prejudges the former of these issues—XIV, p. 21) is that he regards all explanations in terms of unconscious mental processes to be causal.

Freud was very much alive to the variety of questions about human behaviour, nor did he in fact deny to people a " plurality of aims " as some have claimed. (See I—Mr. Anderson's claim about the plurality of man's aims and Dr. Peters's claim about the plurality of the sorts of question which can be asked about man's behaviour should not be confused.) What he did was to subordinate the many aims of man to a few predominant ones, thus presenting the character of his activities as being different from what it often appears to be like. His aim was to correct various misapprehensions, but at times he failed to acknowledge certain facets of man's character into which he certainly had an insight. I think that what led Freud to subordinate man's aims and motives to those of deriving gratification from any experience and of avoiding pain was his constant confrontation with such guiding aims in the diverse manoeuvres of his patients in the daily interviews he had with them. He saw that almost all the components which go to make the many forms of human relationship could be and were, in fact, used to these ends. This is certainly connected with the fact that the people who went to him for treatment were those who had failed in human relationships. It is also connected with the fact that these particular features tend to be crystallised (transference and regression) and come into play in those situations provided by the analytic setting, at once permissive and threatening, under the analyst's unswerving interpreting activity. Another reason why he put such emphasis on the guiding influence of a policy to seek pleasure and avoid pain was his vision of the extent to which a rarely acknowledged mentality (item 5 above) influences man's activities. (It is true that Freud's " account of the working of primary processes creaks with causality ", as Peters puts it—XIV, p. 11. But this account needs to be studied in conjunction with much else that Freud said and wrote, and I have advisedly used the term " mentality " in this connection.)

Thus it was not a blindness to the complexity of human behaviour raising many different sorts of question in the minds of those who seek to understand it which made Freud speak in ways which caused the dissatisfactions we have just indicated. It was conceptual confusion which arose in connection with the need to give words to his penetrating perceptions and to indicate their place in an interpretation of human character which at times made his accounts unsatisfactory to the point of giving rise to serious misunderstandings about his vision and intention. Having made this preliminary point we can now turn to the main topic of our discussion.

The notion of unconscious ideas, wishes and emotions has puzzled philosophers for a long time, and the problem of giving an account of how statements about these are verified presents difficulties which have not always been satisfactorily surmounted. To begin with it may seem self-contradictory to speak of a person as having such-and-such an idea and yet not knowing it. As Professor Laird puts it (X):

> The trouble about ideas and wishes and the like, however, is precisely that they do not seem to be anything at all, *except* when they are present conscious facts.

Dostoyevsky's hero too thinks the same, and yet he is irresistibly impelled to assert what seems an absurdity to him (V, p. 124):

> . . . 'Had the idea ever entered his head before, if only as a dream in a vindictive moment ?'
> He decided that question strangely . . . 'Pavel Pavlovitch wanted to kill him, but didn't know he wanted to kill him. It's senseless, but that's the truth,' thought Velchaninov.

Freud has himself stated that the appearance of contradiction springs from an identification of mind with consciousness dating from Descartes (VI, vol. v, p. 167). But he did not always fully grasp the character of this identification and opposed it in many places as one would oppose a prejudice or piece of obstinacy. Yet, in other places, he did recognise something of its character ; at any rate he felt something of its spell to the point of saying that it is " an assumption which seems *natural* " (VIII, p. 16—italics mine). This was an admission of the strength of the spell which he tried to resist by saying that identifying mind with consciousness " is a matter of convention " which is " thoroughly unpractical " (*ibid.*). This was very inadequate.

Briefly, the idea of self-contradiction develops as follows : When as a result of the failure to locate a person's wish (for instance) in what he does or says—for these at best only express his wish, they are not the wish itself—we make it something interior, and therefore observable only to him, the wish seems to vanish into nothingness when we say that it is not observed by the very person whose wish it is. Hence we seem to contradict ourselves when in the same breath we assert its existence. Therefore, a careful examination of the sorts of claim we make when we say that a person has such-and-such a wish will do a great deal to lessen an initial resistance like that expressed by Laird As Professor Wisdom put it in one of his lectures :[1] " The trouble one feels about unconscious mental objects is due to the trouble one feels about conscious (mental) objects ." When such a lot of work has been done recently in philosophy to better our grasp of psychological concepts such as wish, motive, intention, emotion, etc., the initial resistance to the concept of the unconscious on the grounds that it is self-contradictory must have considerably diminished.

Yet it was not simply on account of the fascination exercised by " the ghost " that Dostoyevsky's hero thought his overpowering conclusion to be " senseless ". There *was* a conflict between his reasons for asserting that Pavel Pavlovitch had had the intention to murder him and those for saying that he had not. Such a conflict between reasons for giving one description and reasons for giving the opposite description is characteristic of most cases where psycho-analysts speak of unconscious wishes, motives, and ideas. When, however, we take a close look at such cases, when we consider in detail the claims made in asserting that a person wishes or intends to do something unconsciously, that which at first suggested conflicting statements upon an attempt to sum it up descriptively turns out to be " subtle patterns in time which are hard to grasp " (XX, p. 274).

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[1] Lectures in Cambridge on the Philosophy of Psychology, January 1954.

Another source of difficulty is that in being characterised as unconscious that part of the mind so characterised seems to be excluded from being an object of knowledge of a kind, at any rate, that we are supposed to have of the conscious mind. In saying that the unconscious and its contents are *ex hypothesi* unobservable we make the concept of the unconscious very nearly a metaphysical notion such as Kant's things-in-themselves or noumena. Even Freud seems to speak from such a mood of metaphysical despair when he says that " consciousness yields no evidence " of unconscious acts (VI, vol. iv, p. 99). What leads people to regard the unconscious as *ex hypothesi* unobservable, the inability " to say what the unconscious is " (VI, vol. v, p. 168) has bred a family of views about the logical status of the unconscious which, I believe, stand in the way of a proper appreciation. Among the more wide-spread of these we can list such views as that the unconscious mind is an inference, that it is a hypothesis or assumption—and the latest of these and by far the most sophisticated among them—that the concept of the unconscious is a " theoretical concept " (see IV), like the concept of an electron.

Freud, himself, was led by his own statements to adhere to some of these views though he never felt satisfied with any of them. Also he was not clear about the differences between them and made a rather indiscriminate use of them in his attempts at " justification " (see VI, vol. iv, " The Unconscious ", Sec. I). For instance, when he writes that the unconscious acts of a person " fall into a demonstrable connection if we interpolate the unconscious acts that we infer " (VI, vol. iv, p. 99) is he saying that we infer the existence of " unconscious acts " from " other proofs or signs " (*ibid.* p. 23), or that we postulate them for the order they bring into our vision of (conscious) mental life and for the predictions they make possible?

First about the view that the unconscious is a hypothesis or assumption, that its existence is postulated on account of what it does for our understanding as well as on account of the help it gives us in constructing " a highly successful practical method by which we are enabled to exert a useful influence upon the course of conscious processes " (*ibid.* p. 99). Statements about somebody's unconscious wish or sorrow need not be hypothetical statements any more than statements about someone's conscious distress or intention need be. A statement about somebody's distress is a hypothesis when the distress is far from being apparent though essential to assume if his tears are to make sense. If all the cases where psycho-analysts speak of unconscious feelings and thoughts were no better than this (as Professor Price seems to suggest incidentally when discussing " unsensed sensibile "—see XV) then statements about the unconscious would be hypotheses. A person who maintains such a view forgets that what is concealed from the subject and many others may be as apparent to the psycho-analyst (in analysis) as states of mind, motives, and wishes which we characterise as conscious. He also, surely, ignores all the complex considerations that go into the characterisation of some of the various kinds of modification which take place during the course of a psycho-analysis as " deepening insight "—or the becoming conscious of what was unconscious. (I should like to note, in parenthesis, that regarding the unconscious as an assumption naturally leads to the question why what is assumed is held to be mental. Freud discussed this question at great length, but I cannot take up this issue here.)

Freud's view about the logical status of the unconscious as an assumption or hypothesis, in the reasons he gives for adopting such a hypothesis, makes his account at times come close to the one Professor Braithwaite gave of what he called " theoretical concepts ". This view has recently been developed by A. C. MacIntyre in a little book called *The Unconscious*. As he puts it, on p. 46 of this book, in scientific theorising " concepts which refer to unobservables have a legitimate, important and necessary place. And in elucidating the nature of the concept of the unconscious the possibility that it is a concept of this kind must be taken very seriously " (see XI, pp. 46-48).

Freud, himself, wrote (VIII, p. 14) :

> What will you,—it is a hypothesis, and science makes use of many. The earliest always prove to have been somewhat crude and should be labelled ' open to revision '. I take it that it is unnecessary for me here to appeal to the philosophy of ' as if ' which has grown so popular. The value of such a ' fiction ', as Vaihinger would call it, depends upon how much one can achieve with it.

And somewhere else he wrote (VI, vol. iv, p. 29) :

> The index-value of the unconscious has far outgrown its importance as a property.

Thus, borrowing an expression which Braithwaite uses in connection, with scientific theorising, Freud might have said that statements about unconscious ideas and wishes are "conceptual devices" (IV, p. 339) by means of which psycho-analysts are able to organise their knowledge of the behaviour and (conscious) experiences of their patients and to make predictions. Having been led, by imperceptible steps, to present statements about the unconscious as "symbolic machinery", in the sense in which Ogden and Richards have used the expression (XIII, see for instance pp. 188-189), he made statements about the unconscious given during analytic interviews too theoretical for his own liking. Thus he writes (VI, vol. v, p. 109):

> Janet too had made use of unconscious acts in mental life; but, as he insisted in his later polemics against psycho-analysis, to him the phrase was no more than a make-shift expression *une manière de parler*, and he intended to suggest no new point of view by it.

No doubt to speak about electrons is more than just a way of speaking about flashes of light, as Braithwaite has unmistakably shown us. Hence on the view which makes the concept of the unconscious into a theoretical concept to speak about what a person wishes or feels in his unconscious mind is not just a way of speaking about what he does and undergoes. But, before arguing against the view that the unconscious is a theoretical concept, let me attempt a brief summary of what in Braithwaite's view a theoretical concept is:

It is a concept which occurs in the higher-level propositions of a hierarchy of hypotheses organised in such a way that the lower-level propositions are deducible from the higher-level ones. These propositions are related to one another as formulae in a calculus. Hence, it may be said that together they represent a calculus and thus constitute a deductive system. The formulae which are thus related to the lower-level propositions of the system are given direct meaning; they are thus observation-statements. Those from which these formulae are derived are interpreted simply as propositions so related to the ones already interpreted as observation-statements; thus they are given indirect meaning. So the least derivative formulae of the calculus are interpreted last, *i.e.* they are "fitted to the deductive system derivatively in virtue of their place in the calculus". These are the formulae containing the symbols interpreted as theoretical concepts. What makes these concepts empirical is the connection the statements in which they occur have to observation-statements by virtue of the system they constitute. Thus " to say that theoretical concepts exist is (no more than) to assert the truth of a theory in which they occur ". Words which stand for them " may have independent meaning in some subjective sense of meaning ; I may think of an electron as a minute sphere . . . but this is not how the words are used in a treatise on physics ".

We cannot, therefore, think of theoretical concepts apart from calculi interpreted as deductive systems. The virtue of fitting the observation-statements of scientists into such a system is that in doing so we make it possible for the direct evidence for each of these to become indirect evidence for other observation-statements—actual (thus explaining what they state) and possible (thus predicting them).

Statements about a person's unconscious mind are not on a different level from statements about his conscious mind, as they would have been if the concept of the unconscious were a theoretical concept. Both sets of statements may be said to *describe* his mind. But statements about electrons and fields of force do not constitute any part of the description of flashes of light, etc. They only *explain* these, in the way we have indicated. What makes statements about unconscious wishes and motives explanatory is not different from what makes those about conscious ones explanatory. A person who fails to understand and explain a certain piece of behaviour (his or another person's) because he has not recognised what is claimed when he or the other person is said to have this unconscious motive or that unconscious emotion is not like the person who fails to understand or explain some observed occurrence because he has not heard of atoms and electrons, and not learned the atomic theory. He is rather like a person who fails to understand and explain what he observes because he has not observed widely enough ; he sees only part of the picture.

Hence, an account of the logic of the unconscious which makes statements about unconscious thoughts and feelings meaningful only by virtue of the relation which sentences expressing them have to others in a calculus interpreted as a deductive system—*i.e.* only indirectly—will not do. I would like to insist that when a psycho-analyst makes some headway in getting his patient to see the truth of his statements about the patient's unconscious—*i.e.* when he succeeds in making his interpretations " get across " there is no question of anything taking place even remotely describable as the patient accepting a theory. As a matter of fact analysts fastidiously distinguish between true (or genuine) insight and its many bogus varieties which include the case where the patient arrives at conclusions about himself by deducing these from the conjunction of his observations of himself and the theory he has come to accept.

What I wish to oppose is the categorical distinction drawn by such an account between how statements about conscious thoughts and feelings mean and how those about unconscious ones do. No doubt, to talk about a person's conscious feelings and wishes is to indicate various connections and to make various claims about what has gone on before and what is to come. And no doubt such statements as " he feels this " or " wishes that " make the claims they do because much of our common stock of knowledge about human beings is brought to bear on the particular case in hand. Discussing the meaning and mode of verification of statements like " Big Ben points to 12 ", Wisdom points out that " the full account (of the claims made by a simple statement of this sort) involves all the laws of nature (not excluding those about the effects of " grit in clocks and defective speedometers ") which once they are ascertained are put into the meanings of words " (XIX, p. 214). So one may wish to say that the concept of conscious thoughts and wishes too is theoretical. Wisdom once pointed out to me in private conversation that it may be said that all concepts are theoretical. Thus my quarrel is not with those who after having said that the concept of an unconscious wish is a theoretical concept are willing to allow that the concept of a conscious wish is also a theoretical concept. What I oppose is the view which claims that the logic of statements about a person's unconscious mind is like the logic of statements about the number and behaviour of the electrons in a given piece of matter.

If I understand rightly Braithwaite's analysis of theoretical concepts, one cannot talk of seeing an electron (not even as a possibility one might wish to speculate about) without sinning against logic ; for if the concept of an electron is theoretical (in Braithwaite's sense) then seeing cannot have a place in statements in which the concept of an electron has a place. The proper place of seeing is in lower-level statements. To put it in Braithwaite's words : " As the hierarchy of hypotheses of increasing generality rises, the concepts with which the hypotheses are concerned cease to be properties of things which are directly observable, and instead become theoretical concepts." So while we can talk of observing the effects of electrons, the expression " seeing an electron " is at best a misleading locution. The same would be the case of " making the unconscious conscious " if the concept of the unconscious were a theoretical concept.

As we shall see further down, this phrase—which Freud used to describe the aim of his efforts as a psycho-analyst (VII, p. 363)—is far from being a misleading locution.

Freud, as we have seen, also took other views concerning the status of the unconscious as a concept. He maintained, for instance, that the existence of the unconscious (of unconscious processes) is an inference, that statements about the unconscious are inferences. In other words, he sometimes claimed that what we know about unconscious mental processes we know by inference. He writes (VI, vol. iv, p. 123):

> Thus an unconscious conception (*e.g.* a thought, wish or emotion) is one of which we are not aware, but the existence of which we are nevertheless ready to admit on account of other proofs or signs.

Freud supports this view by comparing our knowledge of unconscious mental processes on the one hand with our knowledge of the external (physical) world and on the other with our knowledge of other people's feelings and thoughts. To begin with the first comparison. He writes (*ibid.* p. 104):

> In psycho-analysis there is no choice for us but to declare mental processes to be in themselves unconscious, and to compare the perception of them by consciousness with the perception of the outside world through the sense organs.

This comparison is rather ambiguous. On the one hand it feeds on the metaphysical difficulties which have led some philosophers to dissociate the perceptible qualities of physical objects from the objects themselves and to claim that since we can only know directly the perceptible qualities we can know the objects themselves only by inference. Thus further down (on the page from which the above passage is quoted) he writes that " the assumption of unconscious mental activity . . . seems to be an extension of the corrections begun by Kant in regard to our views on external perception ". I will not make any comments on this except to say that some of the views about the unconscious being a causal background to conscious mental activity may, in part, be traced to the misconceptions stemming from it. This comparison, on the other hand, suggests that our knowledge of our own minds is like our knowledge of the external world, and that we know unconscious processes by inference just as we know that somebody is approaching from behind from the shadow he casts on the wall.

Wittgenstein is said to have suggested that this way of talking is misleading because it makes us think that the difference between conscious mental objects and unconscious ones is like the one between seen and unseen chairs (XII.) He was certainly right. Freud did sometimes talk in ways which were apt to make the logic of unconscious mental objects too much like that of hidden objects. The striking characteristic of unconscious wishes and thoughts is that they are mainly hidden from the person whose wishes and thoughts they are; and they are hidden in a way which is somewhat different from the way objects are hidden from one by being on the other side of an opaque wall. This way of presenting their hiddenness and the claim that our knowledge of the unconscious is an inference are very closely connected. Wittgenstein was also right about how this way of looking at the matter is apt to bedevil our understanding of the concept of an unconscious mind, as we shall see further down in our consideration of what is involved in becoming conscious of what was unconscious.

Freud also compared our knowledge of the unconscious with our knowledge of other people's minds, maintaining that both are instances of inferential knowledge. He writes (VI, vol. iv, pp. 101-102):

> The assumption of an unconscious is, moreover, in a further respect a perfectly *legitimate* one, inasmuch as in postulating it we do not depart a single step from our customary and accepted mode of thinking. By the medium of consciousness each of us becomes aware only of his own state of mind; that another man possesses consciousness is a conclusion drawn by analogy from the utterances and actions we perceive him to make, and it is drawn in order that this behaviour of his may become intelligible to us . . . the assumption of a consciousness in him (*viz.* in our fellow-man) rests upon an inference and cannot share the direct certainty we have of our own consciousness.
>
> Now psycho-analysis demands nothing more than that we should apply this method of inference to ourselves also.

In so far as Freud had in mind our knowledge of other people's unconscious minds—*i.e.* our knowledge of other people's feelings and thoughts which they do not recognise—it is certainly true that it is not different in character from our knowledge of their feelings and thoughts which they do, themselves, recognise. But to say that we know that other people have feelings by analogy, or that we know what other people feel by inference will not do. Why it will not do to say the former has been sufficiently demonstrated to us by Wisdom in *Other Minds*. In connection with the latter claim it may be worth while to remind ourselves what Ryle has elucidated at great length: that to understand another person's mind is not to infer the occurrence of episodes observable to no one except the person concerned.

Neither will it do to say that we only know our own states of mind, as Freud showed us to have known only too well in his answer to Fliess who in one of his letters accused him of reading his own thoughts into those of his patients. To say that " by the medium of consciousness each one of us becomes aware only of his own states of mind " is either a tautology or a despairing confusion reminiscent of the claim Epictetus made : " Men are not influenced by things but by their thoughts about things." Such a claim stems, in part, from the view that for anything to be known it must cast a shadow-idea in our minds through which it becomes then indirectly known. So, what is really known is only the shadow-idea, never the thing itself. I would like to point out, in passing, that at the source of such philosophical confusion there sometimes is (though I do not think this was so in Freud's case) a genuine and authentic experience of loneliness (*cf.* III), of " being enveloped in, surrounded by our own soul " (XVI, pt. i, p. 115) as Proust has so aptly described it. But the condition which brings forth " perpetual discouragement as we hear endlessly, all around us, the unvarying sound which is no echo from without but the resonance of a vibration from within " (*ibid.*, see also p. 111) is a morbid condition on which psycho-analysis has shed much light.

The passage in which Freud compares our knowledge of unconscious processes with our knowledge of what other people feel and think is apt to suggest that our own unconscious mind is to us as other people's minds. Such a suggestion, apart from entirely obscuring the peculiarity of the concept of oneself which also characterises the concept of one's own unconscious mind, tends to reinforce an attitude of irresponsibility towards that part of oneself which the analyst talks mostly about when one is being analysed. I only appreciated more fully the various facets of this irresponsibility during my own analysis. It is in part the same thing which Ryle has in mind when he writes (XVII, p. 103) :

> Nothing would surprise us more than to hear him say 'I feel depressed' in the alert or judicious tone of voice of a detective, a microscopist, or a diagnostician, though his tone of voice is perfectly congruous with 'I *was* feeling depressed' and '*He* feels depressed'.

To elaborate this point here would take us far afield. I shall, therefore, content myself simply with pointing out that there are special temptations in the case of the concept of my own unconscious mind to overlook the peculiarities which we are not so likely to overlook in the case of the concept of myself—even if in this latter case too we may not succeed in saying satisfactorily exactly what this peculiarity consists of. Freud never really meant to deny this peculiarity. As he explicitly puts it (VI, vol. iv, p. 103):

> A consciousness of which its possessor knows nothing is something very different from that of another person.

This may be stated more clearly if paraphrased as follows: An aspect of the mind of which the subject whose mind it is a part of knows nothing is to the subject something very different from that of another person—whether the latter be conscious to the subject or not. To say that the subject finds out about it (*i.e.* his unconscious wishes and emotions) *in the same way* that he finds out what another person wishes and feels (see Ryle *op. cit.* and Freud, VI, vol. iv, p. 102) by no means goes against what I have been trying to bring out here.

At the cost of a charge of digressing I would like to put in a cautionary remark here. To say that I find out what is in my mind *in the same way* that I find out what is in another person's mind is to say something very important. Yet having said this we may tend to forget what Wisdom has taken great pains to show us—" that the peculiarity of the soul is . . . that it is visible only to one " (XIX, p. 226). I would like to refer the reader to the discussion on pages 226-235 of his paper on Ryle's *Concept of Mind*, particularly where he considers " the facts covered by the words ' The soul is visible only to one ' ", where he is trying to locate that feature of our knowledge of the mind of others which has misled some to say that we never know the minds of others. It is of particular importance in a discussion of the unconscious, and hence the mode of verification of statements about another person's unconscious mind, to see how this feature of the concept of mind may combine with the fact that psycho-analytic interpretations are about what the person they are given to is not aware of, to breed despair and scepticism—for if we cannot be sure and he doesn't know then how can anyone know? The tension between what tempts one to say that A is always the final judge about the truth of statements about his mind and what leads one to say that he has no special access to any of the evidences on which the truth of these statements are based, that his word—whether it be one expressing conviction or dissent—is subject to just the same sort of contextual interpretation that his behaviour and other verbal reports are, can only be resolved by a very careful examination of (*a*) how A may try to mislead B in his (B's) attempts to find out something about A which A knows, and (*b*) how A may co-operate with B in his (B's) attempts to find out something about A which neither of them know. This tension is well brought to the fore by Miss Anscombe in her discussion of the various ways in which we can contradict someone else's statement about his own intention in doing an action (II, pp. 43-49). On page 36 she says that " a man's intention in acting is not so private and interior a thing that he has absolute authority in saying *what* it is—as he has absolute authority in saying *what* he dreamt ". But further down, on page 43, she adds that there is an area in which there is no check on a person's truthfulness. " There is a point at which only what the man himself says is a sign ; and here there is room for much dispute and fine diagnosis of his genuineness " (*ibid.* p. 44). To sum it up : There is a sense in which a man is the final judge about the truth of statements about his mind. However, the more complex the claims made by these statements, the more these claims are about the future, the more symmetrical will their mode of verification become as between him and others. Also, the more scope there is for interpretation of the psychological material on which these statements are based the more will there be scope for personal bias on the part of the subject.

To return to the claim that statements about the unconscious are inferences. After having argued that this will not do as a description of their character I should like to point out where the element of inference lies. In a paper (VI, vol. i, p. 19) where Freud gives a partial elucidation of what made him talk of unconscious mental processes, he refers to the case of an hysteric who is crying, etc., without knowing what he is distressed about. Freud suggests that he is distressed by a past event which he cannot recall and that in his unconscious mind the hysteric knows this. The final proof of this claim is a conscious remembrance of the past event by the hysteric—granted that he also comes to appreciate the connections which are brought to his attention by the interpretations which precede and follow the remembering. It is this which Freud infers, *i.e.* that a change—to be described in such-and-such terms for such-and-such reasons—will come about. What is inferred, then, is not that in his unconscious mind the hysteric remembers, etc., but that he can become aware of it under certain conditions. Yet Freud's comparison (see p. 457) between knowledge of the unconscious and knowledge of other people's minds brings him very close to talking of *interpretation*. Thus a little further down he writes (VI, vol. iv, p. 102) :

> We understand very well how to interpret in others (*i.e.* how to fit into their mental context) those same acts which we refuse to acknowledge as mentally conditioned in ourselves.

I would like to develop the hint Freud gives us here in the positive account which I shall put forward in the second part of this article.

We have seen in our discussion that the trouble one feels about the concept of unconscious thoughts, feelings and wishes is, in part, connected with one's not having a proper grasp of the concept of conscious thoughts and feelings. To say, for instance, that someone is torn with feelings of envy and jealousy is to make many claims about him which go beyond his present behaviour and those parts of his present awareness in which what these claims are about have not come into connection—*i.e.* fragmentary thoughts, feelings, and sensations. To say that he is feeling jealous of so-and-so (consciously) is, of course, not simply to say that he is behaving in such-and-such a way. But neither is it simply to say that he has such-and-such fragmentary thoughts, feelings, and sensations. It is also to claim that in his awareness he has brought them into connection and grasped the significance of the mental state he is in. To be aware of one's jealousy admits of degrees, and the more aware a person is of all that is claimed about him when he is said to be jealous the greater is his awareness of his jealousy now. There are many ways in which he could be prevented from becoming aware of it; and if he has motives for not recognising it he may make subtle use of these. For instance, as a result of there not being suitable rivals in his immediate environment his feelings may not find opportunities for direct expression. Alternatively, too great a fragmentation of thought, or control of internal monologue and restriction of flights of the imagination (or other kinds of mental manipulation) may hinder a correct assessment which constitutes, in part at least, an awareness of the feeling. To grasp what is involved in such an awareness one may find it enlightening to consider the various ways in which novelists convey what their characters are feeling. What sorts of thing do they mention in order to convey to the reader what their characters feel ?

So, in the beginning of this section, I would like to insist on two points :

1. Unconscious feelings are *not feelings which no one knows*, they are feelings which the person whose feelings they are does not know.

2. They are *not feelings which the person whose feelings they are cannot know*, except in the sense that one cannot know what is hidden or what is in the future.

If we allow that A can know what B is feeling consciously, there is no reason why we should not allow that he can know what B is feeling unconsciously. Psycho-analysts often do. I have already pointed out that for A to know that B is envious means that he sees certain patterns in the behaviour of B which he observes in given situations and also hears about, and in the thoughts and feelings which B reports to him. Obviously he has to take into consideration the various complex factors which come into play in B's reports and behaviour. For instance, he has to tell how much conscious working out there is in B's reports about himself, to assess how much the emotional awareness reported is an unsophisticated record of spontaneous expressions of feeling and how much it is the result of B's interpretative activity. He has to distinguish between genuine emotion and its fake varieties. With an understanding of B's motives he should detect the kind of selective activity at work in B's reports. We shall see further down what is involved in this sort of appreciation in greater detail.

Of course, the patterns which A grasps in seeing that B is consciously envious and the ones he grasps in seeing that C is unconsciously envious are not the same. The latter has those features which make us say, for instance, that C's feelings are much less directly expressed than B's, that C disguises his feelings and biases his appreciation in many different ways. And further, of course, the latter lacks those features which make us talk of B's feelings as conscious. As we shall see further down, when C becomes aware of his envy the patterns which made A speak of C's unconscious envy are themselves modified.

One reason why sometimes people do not allow that we can know the feelings of another which he himself is not aware of is that knowing someone to feel anxious, angry or envious is thought to be like seeing the reflections of what that person sees because they characterise his awareness like ripples on the surface of an expanse of water. Thus, when we talk about feelings which the person whose feelings they are is not aware of, it is assumed that we must be talking about something even the reflections of which we cannot see. Here the difficulty about having feelings yet not knowing them combines with the one about knowing other people's feelings. Thus a person who cannot but know his feelings cannot know those of another—at least not in the same way. He can only know them by inferring that they are there and assuming by analogy that they must be of such-and-such a kind. So when we talk about another person's unconscious feelings we are talking about something which we cannot know even in the way which we know other people's conscious feelings. When, now, what makes people sometimes talk like this combines with what makes them say that I cannot know my unconscious feelings the web is spun.

Thus to summarise : The difficulties which many people have felt about the concept of an unconscious mind can be traced, in the main, to the conceptual confusions which sometimes have led them to say :

1. That feelings cannot be unconscious (A). The concept of an unconscious mind is self-contradictory.

2. That I cannot know my unconscious feelings (B), and that others cannot (A′), so that no one can. Knowledge of the unconscious mind is self-contradictory.

We have already seen that we *can* know another person's unconscious feelings just as much and in the same way as we can know his conscious feelings. Now to come to difficulty B. If we allow that a person can know what is hidden there is no reason why we should not allow that he can know his unconscious feelings. Of course when a person finds out a hidden object it is no longer hidden. When we shall know what the future has in store for us it will no longer be knowledge of the future—it will be knowledge of what was in the future. One could say that what was unknown—because hidden or in the future—is now known. But we can ask : Was there (or is there) ever a hidden object ? Is the object here and now (discovered) proof of there having been a hidden object ? Is the hidden object (inferred) the same as the object found ? I do not intend to take up these questions here or to investigate what breeds such scepticism. All I want to say is that we do talk of hidden objects (*e.g.* the cat behind the curtain, the magnet in the casket) and to suggest that we can do so because we have criteria for identifying the object, no longer hidden, with the hidden one. We often, however, disguise the complexity of such criteria and how they vary from one case to another by picturing to ourselves a deity, as it were, who constantly watches every interval, and for whom there is nothing hidden, and no past and future. Not alive to the complexity and variation of identifying-criteria in general, not at home with the sort of criteria for saying " he feels this " or " thinks that ", we feel at sea with the idea of identity between what makes an analyst speak about such-and-such unconscious feelings in his patient and the feelings which invade the latter's consciousness after analytic work has taken place. If we can grasp all that the patient is made to grasp by means of the interpretations which precede and many which follow the appearance of feelings unfamiliar and even, for some time, alien to the patient we shall have grasped the unity in those changing patterns which constitutes the history of a feeling, wish, or state of mind.

Free now, to some extent, from the difficulties which lead one to regard statements about the unconscious as statements about a mysterious realm of the mind, below that surface we call consciousness, knowledge of which is self-contradictory, or to regard them as no more than " *une manière de parler* ", whatever the justifications for it, let us take a closer look at the sorts of thing which make psycho-analysts speak of, for instance, unconscious envy in a person. What sorts of claim do they make about such a person in saying that he is unconsciously envious ? What constitutes his envy and what his lack of awareness of it ?

Looking at such a person (1) we may be struck at first with what would support statements to the effect that he has feelings one would least expect to find in an envious person. He may appear as friendly and capable of generosity and gratitude. (2) We may also be struck by the absence of reasons for such a person to be envious. He may, for instance, be rich and successful, not lacking in what he might have been envious of in another. (3) We may fail to notice some of the things we may have been led to expect by the statement that there is envy in his heart. For instance, whereas we were expecting to find his relationships riddled with disturbances we may find a person suave, friendly, and seeming to get on well with others. (4) On asking him to recall occasions on which he felt envious we may hear him say that he cannot recall such occasions, or that even if he can that he feels now to have changed to such an extent that the person who had these feelings is to him now almost another person. (5) At a closer look, however, we may be struck by how much design there is in all this, how little it seems to be the simple and direct expression of life. We may be led to recognise this, and how much spontaneity is sadly lacking, by noticing a certain rigidity in his behaviour and by the absence of fulfilment. We may come to see that his friendliness is somehow contrived, not genuine, almost serving an obscure ulterior motive instead of being gratuitous—*i.e.* that it is arranged. We may get the impression that his friendliness provides him with occasions for self-congratulation almost verging on triumph, or merely that its exercise is a source of satisfaction to him towering above the considerate awareness of the person towards whom it is directed. Or one may come to see that its exercise is an anodyne to anxiety—even if the sources of the anxiety are far from clear. (6) We may be struck by how little some of the things which his words, some of his behaviour, and his circumstances lead us to expect are forthcoming. (7) We may also be struck by the extreme character of what would lead one to believe that there is no envy in his heart. One may get the impression that any possible sign which is feared lest it gives the show away has been obliterated; the words may come to one that 'it is all too good to be true'. (8) In the evidence we assemble we may see reasons for saying that he avoids certain situations and actions. For instance, he may respond to certain situational changes by an anxious withdrawal or by actions designed to prevent them when such changes become ominous. (9) These may bring to our attention the inordinate amount of energy he spends in order to maintain what he has established. We may then begin to see how much he restricts his life by thus pursuing a policy of withdrawal and denial. (10) That which we ignored at first because our first dominating but superficial impression excluded it and which later disturbingly—for the sense our unity of vision conferred on the patterns we observed—began to come to our notice threatening that sense, now once again gives promise of a new sense. We may, for instance, begin to see some grounds for the dissatisfaction which at first seemed to have no grounds. We may begin to see conflict and inconsistency of behaviour and at the same time begin to understand how these come about. With a better understanding of the different types of opposition between sets of feeling—ambivalence and conflict; reaction-formation and other mental manipulations—and with an increasing aliveness to the "over-determination" of feeling we may begin to develop a greater appreciation of the sorts of reason for saying of a person that he feels this or that. This may help to cultivate in us a greater caution towards first impressions threatening to solidify into categorical views by being shared and reiterated, as well as an open mindedness towards alternative interpretations. (11) Looking now more searchingly at what begins to change face under our defeated vision and developing new understanding we may begin to see—as one sees the snake among bushes, at first unseen though there from the beginning—indirect expressions of envy. For instance, after noticing the contrived character of his friendliness, and the compulsiveness with which he seeks people's friendship, we may begin to see how much he does so in order to deprive others of what in his feelings he lacks and to secure it for himself. Certainly our growing knowledge of the person's motives and intentions does a lot towards reorganising our vision. Thus we begin to see that the policy which we began to appreciate as one of denial is also one of seeking gratifications motivated by envy. (12) This introduces further sense into the patterns which emerge, in their various aspects, in the person's behaviour and in his reports about himself. For instance, the guilt and depression which we also come to recognise begin to make sense in the light of our awareness of the envy and our increasing understanding of what it feeds on.

If now, after this, one is still inclined to say that the statement that someone is unconsciously envious is a hypothesis which draws our attention to, and best explains, the various facts about this person the kind of which we have considered, I have three suggestions to make in order to show how inadequate such a description is :

1. What here is referred to as " facts explained by a hypothesis " is less the sort of thing that emerges in a scientific experiment and which is explained on the basis of a theory than what emerges as a result of a changing vision. I have already tried to indicate how such a change of vision may come about in a piecemeal way, and how in such a development of new awareness seeing the detail assembled in a new light and assembling new detail is fused together. Here it may be said that the facts grow in the detail assembled, with the interpretations which net them. The facts and their intelligibility are much more intimately linked here than elsewhere.

2. The statement about unconscious envy is a statement about what can be seen in the same way that the snake hidden in, but not behind, the bush is seen. It would be a highly sophisticated piece of eccentricity to say that the statement " here is a snake " is a hypothesis which draws attention to certain visual patterns and best explains the way in which these change—*e.g.* when the snake decides to move.

3. When the person who is said to be unconsciously envious comes to accept and understand what is said he does not simply find himself in a privileged position to understand certain facets of his behaviour, thoughts and feelings in the way that the physicist does who begins to make sense of what happens in tubes and on reading clocks because he learns and accepts a theory. As we shall see presently such a person comes to do more than simply grasp the indirect expressions of envy ; he becomes a person who is consciously envious, not needing to disguise the expressions of his envy, and fully conscious of the grip which

at times the feeling gets of him. In other words, it is not simply his vision of the patterns which change but also the patterns themselves.

Freud described this latter change as the emergence of the hidden creatures of the mind. It will be remembered that he emphasised that " the mental, like the physical, is not necessarily in reality what it appears to us to be " (VI, vol. iv, p. 104). In this way Freud intended to stress the third dimension in the mental world which his work had revealed.

Ryle and Wisdom have done a great deal towards giving us a better grasp of what it is to make mistakes about one's own mind. They have shown that these mistakes are failures to recognise patterns in time, and Wisdom has shown how like these are to failures to recognise patterns in space. Freud showed us how the openness of the mental world to misapprehension (linked with how it is known) may be systematically exploited by people, when they have motives for avoiding recognising what they feel or wish. He showed us how as a result of the modifications paving the way towards a biased appreciation and resulting from it, what makes such people fail to apprehend what is in their own mind is more like hidden creatures which may be made to emerge than like creatures whose presence we fail to take in, though they are before our very eyes. It is one thing to misinterpret one's own feelings when they are expressed directly, another thing to have false ideas about what one feels because one disguises it from oneself. This gap is considerably widened when the disguise has gradually grown through a lifetime.

If we can grasp the variety of ways in which a person may bias his appreciation of what he feels, thinks, and wishes, and how the bias grows to its full stature in stages, we shall appreciate better why the true but unrecognised feelings, etc., are described in terms appropriate to objects hidden in the third dimension. To this end it may be worth while to consider how such bias is undone and the hidden creatures emerge.

When a person is successfully analysed part of the evidence through which he recognises what he has till then been unaware of is often as alien to him as when he comes to know a person who has kept himself a stranger. Knowing oneself is often a much more difficult thing than knowing another person, especially considering how much more opportunity one has to know oneself. Still, it should be obvious that even though a person has systematically misled himself and biased his appreciation of what is in his mind, he has all the grounds to correct his appreciation by working it out, as it were, step by step. If he lacks evidence it is still he who keeps it from himself (by avoiding confronting certain situations, for instance); if he is not aware of doing so, he still has grounds for appreciating that he is arranging the evidence, controlling it, or keeping it at bay. It is mainly on account of this that it has been said that " trying to understand the unconscious motives of any human being is like trying to get at the middle of an onion, or digging through layer upon layer of geological strata " (XVIII, p. 51). Of course such analogies also illuminate that the stages through which self-bias grows lie in different periods of the individual's life history. But, such analogies have, as Wisdom points out, led people to " think of unconscious mental events as behind, below, deeper down than, the conscious ones as there are deeper and deeper depths in a stream " (XX, p. 279).

We have seen what is misleading in the idea that the difference between conscious and unconscious wishes and feelings is like the difference between seen and unseen chairs—*e.g.* that what gives us justification for speaking of unconscious feelings and wishes is related to the feelings and wishes we are referring to as the shadow of an unseen chair is to the chair itself. People who understood this tried to counteract the misleading influences of the analogy of objects hidden in the third dimension by that of patterns we fail to take in. Freud, himself, stressed this counter-analogy when he talked of the case of the student who may look through a miscroscope yet " see nothing, although it is there and quite visible " (VII, p. 365). We have now pointed out how close seeing what is in one's unconscious mind may come to seeing what was hidden in the third dimension, and in what respects the former parallels the latter. To say that seeing what is in one's unconscious mind is like seeing what has been before one's eyes all along is useful in that it frees us from " the evil effect of the cramping idea that every surprising, revealing, statement must be justified either by giving information which the person surprised lacks or by taking him through a process of strict deduction which he has been incapable of carrying out (which) is not so easily detected in our judgement of what psycho-analysts say " (XX, pp. 269-270). Yet, it may also obscure the variety in the changes involved in cases where we speak of increasing or deepening insight. Further, again as Wisdom says: " Part of the justification for things psycho-analysts say which surprise us *does* lie in information which most of us have not possessed. A metaphysician never tells us anything we haven't heard before. But psycho-analysts do. . . . And though Freud works very much by recalling and connecting things familiar to us he also tells us astonishing stories " (*ibid.* p. 270).

Let us now concentrate on the sorts of change covered by the expression " becoming conscious of the unconscious ". What are the claims made when it is said that a person has become conscious of what was unconscious ? Why are these changes described from one angle as the emerging of what was hidden, and from another angle as the removal of blindness ?

Supposing that a person who thinks of himself as extremely friendly, loving and tolerant (an " angel ") for some reason goes to analysis and finds his analyst interpret much of his analytic behaviour as the expression of anger and hatred. What would it be like for this person gradually to see the truth of these interpretations and to become aware of his anger and hatred ? No doubt the course which such insight takes will vary from one person to another, but it would be a fairly common occurrence for such a person, after some analytic work, to begin to see through what he took to be friendly feelings, and to begin to apprehend some of his " angelic qualities " as a façade, and himself—in that respect—as a fake. Once he sees the pretence character of what he took to be friendliness, and begins to desire to be more sincere, whatever the price, he will no longer deceive himself *vis-à-vis* those demands and frustrations which he made an effort to meet with tolerance. He will now react to them more openly with anger. We may describe the change here as the giving up of a disguise policy and a coming out into the open.

But these modifications may be less transitional and more abrupt in character. For instance, gradually worked up into a state of mind which the patient apprehends as one of taxed patience he may express his anger and hatred in the violence with which he denies interpretations to the effect that he is hostile to an analytic approach. Here his friendliness abruptly slips from him, his tolerance collapses. But the insight which the analyst is trying to impart may take long to come, the patient swinging between all that comprises his familiar state of mind and these outbursts the character of which he fails to apprehend in the extreme self-righteousness which accompanies them. The patient acquires further insight when, as a result of more work, he begins to see how little these outbursts are appropriate to the present situation, how and why they have been controlled in the past, and how intimately what is responsible for them is connected with his aims, motives, wishes, and ideas, and dates back to his past.

We see then that in what leads to confrontation with the unconscious there is a modification which is like a change from feeling one way (*e.g.* friendly) to feeling another way (*e.g.* unfriendly). There is also a modification which is like a change in appearance where there is an underlying continuity which is seen. This latter resembles the case where we take in a pattern and realise that it has been there all along. The former may be described as *a change in feeling*. But we must be careful to distinguish it from such cases as the one in which the infant's love for his mother gives place to hate when she withdraws her breast. The friendliness which slips or is given up is seen to be not what the patient really feels, but rather an arrangement to camouflage his true feelings. (This does not mean, as he may think then with dismay, that there is no love in his heart to make him capable of friendliness. But it will take more work for him to see it, and a willingness to pay a greater price.) The latter may be described as *a change in vision*. In this the patient grasps what confronts him now (*viz.* what was unconscious) as having been in the past, as something which is not new. This is more than remembering forgotten past occasions on which he felt as he does now ; it is seeing the past in a new light. This is like the case where someone wakes up at night and sees something which later, in the daylight, he discovers to have been something other than what he had taken it to be. This helps him to recognise how intimately the person who felt as he now remembers him to have felt on past occasions is himself.

In those cases where collapse and confrontation long precede a change of vision, the former brings the latter because of the interpretations which have been given prior to it and because of many which follow it. The gist of what these do for him may be conveyed by saying that they tend (i) to push the responsibility for the emotional response from the present external situation to the core of the patient's personality, and (ii) to present the emotional response as more or less a replica of numerous past instances constituting its history (itself a trend in the history of the patient's self). If we can grasp all that the patient is made to grasp by the help of these interpretations we shall appreciate the point in describing these changes as the becoming conscious of what was unconscious. But it is not always the case that confrontation precedes appreciation. Sometimes the change in vision paves the way to a confrontation which is resisted at every step. This, for instance, is the case with the person who realises how much of what he feels is sham, and gradually learns to recognise the true nature of his feelings and to express them more directly. But usually the two are so intricately fused to one another that it is very difficult to tell them apart. Of course, there are also cases where the two changes come abruptly and at one and the same time.

Similarly, in grasping all that the patient is made to see by the interpretations which he is given we shall appreciate why the patient's dawning awareness is described as the removal of a previous blindness, and why Freud describes the knowledge which comprises it as coming *per via di levare* and not *per via di porre*. This latter would involve us in an examination of the working of analysts, so I cannot take up this issue here. (In particular, see descriptions of analytic work on resistances—*e.g.* in Wilhelm Reich's *Character Analysis*.)

We see then that the expression " becoming conscious of the unconscious " is far from being a misleading locution, and the point of describing the changing patterns in a successful analysis in terms of seeing what was in the mind, until then unseen. Of course, not all the changes in a successful analysis are covered by this expression which mainly refers to a progress in insight. Besides these there are all those changes described as " working through ", " integration ", " character-transformation " and other therapeutic modifications. No doubt there are no hard and fast boundaries between these, and an examination of these changes may lead to a host of interesting but teasing questions connected with the character of insight-therapy. In parenthesis, I would like to add here that if we can grasp properly the character of all those changes making progress in insight possible, involved in it, and in turn made possible by it, we shall have made a good start in getting clear about the difficulties giving rise to these questions.

In this article, where I examined some of the conceptual difficulties which stand in the way of our appreciation of what Freud meant when he talked of the existence of an unconscious mind, I thought worthwhile to emphasise two things: (i) That some of us sometimes do know what is in someone else's unconscious mind and that this is like the knowledge we have of what our friend feels and wishes when he, himself, knows it. (ii) That one can find out what is in one's own unconscious mind and that this is precisely what psycho-analysts help one to do. I have also tried to show that in presenting the patient's present feelings, brought about by interpretation work, as what was in his unconscious mind the analyst brings about in his patient a new orientation towards himself. To grasp what this consists of we need to be clear about all that the analyst opposes in his patient's way of seeing in order to bring about this change of vision.

In this way we shall see how much Freud " intended to suggest a new point of view " by the concept of the unconscious. Freud was thinking of this new orientation when he complained that though " Janet too made use of unconscious acts in mental life . . . he intended to suggest no new point of view by it " (VI, vol. v, p. 109), and charged Adler with having rejected the unconscious. (See VI, vol. i, pp. 338-341. Also see Jung, IX, p. 51, where he speaks of Adler's neglect of the unconscious amounting to a complete denial.)

A proper understanding of what made Freud develop the conception of an unconscious mind cannot come without an apprehension of detail which may easily be obscured or distorted by the expressions which cover it. It is not, however, only language which may stand in the way of such an apprehension of detail, but also all those motives and anxieties in our heart which comprise bias and illusion. What makes us inimical to having such an outlook may also stand in the way of our appreciation of what it involves. As Wisdom so aptly points out : " The battle with illusion, disillusion and despair is something more personal than this ", *i.e.* the philosopher's battle with confusion and misapprehension (XX, pp. 261-262).

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III.—SAMPLING AND THE PROBLEM OF INDUCTION

By Harry A. Nielsen

Hume's problem of justifying induction lies behind recent as well as earlier attempts to establish a rationale for inductive inferences. Among recent attempts, to mention only a few, are those of Rudolf Carnap, Donald Williams, and Hans Reichenbach. Now judgments based on samples are, as all agree, one kind of inductive inference. In this paper I want to consider the question whether Hume's problem or these attempts to solve it have any application to what all the world calls sampling. The widespread belief that they do apply to sampling conceals, as I will try to show, an error which would have to be called solecism if we did not reserve that term for *self-revealing* misuses of words.

I

Typically, a sample is a conveniently small portion drawn from a lot or batch with a view to judging the quality of the lot in a certain predesignated respect. However, to see more clearly how the word ' sample ' works we must look beyond this definition and notice a few of the other concepts which, as a matter of common knowledge, go with the concept of a sample. To begin with, we pay expert samplers more money than inexperienced ones, careful samplers more than those who are above taking pains. Accordingly, technical handbooks caution the sampler to follow the recommended order of steps rather than grab what falls easiest to hand. Any manual of medical, dairy, or public health sampling procedures will show the labours of sampling spelled out, but these bits of common knowledge are enough to indicate one thing. That is, what many philosophers today would call the *grammar* of the word ' sample ' is quite different from the grammar of ' small fraction ', ' subclass ', ' subset ', and related words. The difference consists in the fact that ' sample ' is used with certain concepts which are not to be found in the company of ' fraction ' or ' subclass '.

One such concept is that of physical force. In relation to drawing samples we speak of mixing, stirring, shaking, and other manual (or mechanical) operations aimed at making the sample-quantity represent the lot. On the other hand, physical force is not a factor when we deal with fractions in simple arithmetic or with subclasses or subsets in higher branches.

Another concept associated with sampling but not with calculating is that of attention to the immediate physical environment. While there is no need for gauze masks or thermostatic controls when one is calculating, certain sampling techniques, as in bacteriology, call for just those precautions if good results are to be expected. The concepts, then, of manual dexterity, the use of force to help make a sample representative, and attention to the local environment, all of which are plainly in evidence on every page of a sampler's handbook, lack application in contexts where we commonly use words like ' subclass ', contexts in which we are reasoning deductively.

My purpose in contrasting the uses of ' sample ' and ' subclass ' is not to draw factual conclusions from the way we ordinarily speak, nor indeed to draw conclusions at all, but merely to notice, first, that the way we speak shows that we do distinguish between those words, whether one prefers to call the distinction factual or something else, and, second, that the distinction becomes plainer when we recall that certain familiar concepts go with ' sample ' but not with ' subclass '.

A related point, which also commits us to nothing beyond itself, comes out in this way. In much of the literature sampling is represented by paradigms which offer to give us the logical formula for any and all sampling inferences. Some are simple verbal paradigms, such as ' in sampling we reason from *part* of a class to *all* of it '. Often they are argument-forms patterned on the syllogism, with blanks standing for all possible variables. Some recent writers, notably Carnap and Reichenbach, incorporate their sampling-paradigms into more general probability calculi. The point I am leading up to is this: when sampling is represented by a general formula, be it as simple or as complicated as you please, the three concepts we discussed above—of dexterity, force, and attention to surroundings—never appear in the formula.

When sampling is represented by a general formula, what sampling *is* (according to the formula) may be put discursively as follows: " Examine a statistically insignificant quantity out of a lot or batch, and from that judge the quality of the whole." This, neither more nor less, is what formulas, as such, of sampling make out sampling to be. The claim made for these formulas is that they capture everything essential to sampling, everything the sampler depends upon for good results. At the same time, however, no general formula has room for an account of the options and pains taken by a sampler in drawing his sample. Therefore, in so far as such representations are said to stand for actual cases of sampling, they invite two questions. First, do samplers depend upon the use of a *formula* for accurate results? Second, are actual sampling inferences substitution-instances of any such formula?

As to the first question, samplers do not depend upon a formula. As every page of every technical handbook clearly shows, what they depend upon are tactics more or less expertly employed to *make* the quality of the sample conform, or nearly conform, to the quality of the lot. The concept of randomness, closely allied with that of sampling, is treated in handbooks in such a way as to bring out the same point:

> . . . it is important to note that randomness pertains to the operation of selecting the sample, and not to the sample itself. . . . The most intimate evidence of randomness is the judgment of the experimenter. The man familiar with the technological aspects of the articles under consideration is in position to be a good judge—perhaps the only competent judge—of whether the selection was . . . unbiased with respect to the quality characteristic under consideration.[1]

In answer to the second question, sampling is not conducted by plugging variables into a formula. Substitution, which in formal logic has been called a 'rule of inference', is not on the list of the sampler's options when the situation calls for a sample to be *drawn*. A contemporary philosopher of induction comments to similar effect:

> . . . an investigator skilled in sampling techniques appropriate to botany may not be able to cope satisfactorily with the problems of sampling in economic studies.[2]

We see this also in the fact that no formula of sampling claims to dispense sweepingly with technical handbooks, or promises to make an expert sampler out of a beginner. This is next to saying that no such formula prescribes the conduct of a good sampling inference. A paradigm of sampling-in-general has no place for riders specifying pains and options to be taken in this or that sampling situation.

We find, then, in many philosophies of induction, something unlooked for: their representations of sampling omit all reference to precisely what the sampler depends upon for good results. The decision, or resolve, to let sampling be represented by a formula is the immediate cause of this omission. I would like to come back to it in the next section.

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[1] Leslie Simon, *Engineers' Manual of Statistical Methods* (John Wiley and Sons, New York, 1941), pp. 161-163. Almost identical remarks on the same point may be found in Charles S. Peirce, *Collected Papers*, ed. Hartshorne and Weiss (Harvard Press, Cambridge, Mass., 1930), vol. ii, par. 381.

[2] William Kneale, *Probability and Induction* (Oxford University Press, New York, 1949), p. 218.

The decision to let sampling be represented by a formula has, as one of its consequences, the near-solecism to which I alluded at the start, which may be brought out as follows. As an exercise, someone might copy out of a technician's manual the instructions for sampling a certain kind of stuff for a certain quality, and in his copying take care to drop out all plain or implicit reference to the pains which the sampler is told to take in order to *make* his sample representative. Two things will be true of the expurgated account. First, it will preserve this much, and no more, of what sampling involves: ' take a statistically insignificant quantity out of a lot or batch, and from that judge the quality of the whole.' That is, it will preserve what all formulas of sampling preserve. Second, the new account, by leaving out all reference to the sampler's pains and options, will treat, in effect, of *subclass* rather than *sample*. The concepts in terms of which we distinguished above between sample and subclass will have been screened out of the new account. In default of their presence, and since only by their use can the sampler be told ' how to go about it ', the new account tells him nothing more than to pick blindly whatever comes first to hand. Such a procedure is sure to acquaint him with a subclass, but it is far from what the world calls sampling.

This exercise has already been done for us. Any general formula for sampling must exclude all reference to the sampler's skilled and watchful use of force in his tactics, with the result that ' sample ' and ' subclass ', as far as the formula is concerned, come to mean the same thing. The blurring of so considerable a distinction would be solecism, it seems to me, except that we only bring that charge against a self-revealing misuse of words. In this instance the fault does not crop out in an ill-chosen word but lies a little under the surface in an ill-chosen mode of representation in which everything that is said is said well enough.

If these remarks are correct, then the traditional problem of induction (as it has been applied to sampling) is a problem of justifying inferences not from samples, strictly speaking, but from small fractions or subclasses received blindly and passively by the experimenter, or in other words considered quite apart from his pains and options in drawing them. Nothing could be more apparent than the need to justify our using the type-formula of such an inference, in case it were true that this is what samplers use. But just the opposite is true, they do not use such a formula. No such formula prescribes the conduct of their work, and every one leaves out of account what does prescribe it. Here, it would seem, are grounds enough for saying that the problem of induction applies to sampling only in so far as a writer allows his mode of representing sampling to screen out what samplers in fact depend upon for accurate results.[1]

II

The distinction between sample and subclass is obscured [2] not directly but as a consequence of the resolve to represent sampling by a formula. That initial resolve would be accounted for if there were some striking visible likeness between, say, calculating the temperature Fahrenheit from a given temperature Centigrade and, on the other hand, determining the butterfat percentage of a shipment of milk by taking a sample. In the absence of any great likeness in the *conduct* of these two kinds of inference, we must look elsewhere to explain the resolve.

As everyone knows, there are quite a few expressions which do double-duty for deduction and induction. Among the commonest are ' knowledge ', ' science ', ' inference ', ' reasoning ' and, not least, ' logic '. These overlapping usages are enough to suggest a greater likeness between deduction and induction than anyone can discover by looking at cases. By itself this verbal analogy deserves little comment. There is, however, for deduction, a kind of model paradigm, that of a substitution argument-form with all positive content omitted in order to display with perfect transparency the relationship between premises and conclusion. The familiar textbook treatment of Barbara and other simple deductive inferences, with proper emphasis on their ' logical form ', invites (in many of the same textbooks) a treatment of inductive inference which closely resembles it in notation and format. From these two factors, namely, the overlapping usages and the choice of a mode of representation which is by and large above suspicion, it is but a short step to the belief that we can capture the 'logic' of sampling just as we capture the logic of Barbara: in abstraction from all matter affecting the *conduct* of the inference. Once this much is supposed, it is no step at all to imagine that the strength of sampling must lie somehow in the formula, as is true of Barbara, or else in cumulative applications of the formula. To be carried along in this way, however, is to fail to see that, while the paradigm of Barbara *prescribes* the conduct of the inference, *viz.* by substitution, the paradigm of sampling is altogether useless for making a good sampler out of a duffer.

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[1] The same reliance on formulas teases writers into talking about sampling in metaphors of blind effort, *e.g.* Reichenbach's ' wager ' and L. Ruby's ' leap in the dark '. The sampler's use of all practicable physical measures to rig the outcome is blocked out of sight by the mode of representation.

[2] In a number of writers the mix-up between samples and subclasses is revealed explicitly. In Donald Williams's *The Ground of Induction* (Harvard Press, 1947, p. 77) we find : " . . . the subclass MQ may be any class included in the class M. The kind of subclass, however, on which our empirical knowledge is in fact based is the observed part of the class M. . . . " And Rudolf Carnap includes this in a glossary : " Sample : a subclass . . . of the population " (*The Logical Foundations of Probability*, University of Chicago Press, 1950, p. 582.)

Since we have but one standard of goodness for bare patterns of arguments, the writer who chooses to talk about sampling in terms of such a formula must, if he is not on Hume's side, try to show that his version of the formula meets that standard in one way or another. One way is by adducing a suppressed premise, a practice no longer very much in the limelight. C. D. Broad's comment is still decisive:

> (If the theory that inductive inferences depend upon a common major were true) . . . the conclusions of all inductive arguments in which the observations were equally carefully made would be equally probable. For what could vary their probabilities? Not the major, which is common to all of them. Not the minors, which, by hypothesis, are equally certain. Not the mode of reasoning, which is syllogistic in each case. But the result is preposterous, and is enough to refute the theory which leads to it.[1]

Another device, used by Donald Williams and others, depends upon the arithmetical proof that, for any finite hypothetical multitude, the great majority of possible subclasses will resemble the total population in the proportion to which they exhibit a given characteristic. High odds, Williams argues, will therefore favour the sampler's getting a representative sample, and these odds establish for sampling inferences "logically cogent implication of intermediate degree".[2] Professor Nagel, in a review, puts his finger on the weak spot:

> (Williams's) demonstration, in order to be cogent, involves the tacit but unsupported assumption that the relative frequency with which samples possessing a certain statistical composition are *actually drawn* from a population is approximately the same

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[1] C. D. Broad, 'The Relation Between Induction and Probability', MIND, xxix (1920), 11-12.

[2] Donald Williams, *op. cit.* p. 55.

> as the ratio of such samples in the sample-population of logically possible samples.[1]

In actual sampling we are up against 'sample-populations' which differ widely as to both accessibility and prejudicial distributions of the quality we are sampling for. Those odds which, in Williams's view, overwhelmingly favour the sampler would therefore go into effect, if at all, only after the sampler had examined his options and applied his techniques. But the instructions for using those techniques are not given in Williams's version of the sampling formula, or in any other version for that matter.

Williams's concept of 'logically cogent implication of intermediate degree' reflects a popular tendency to see in paradigms of sampling the forms of *probable* inferences. In keeping with this tendency, Reichenbach and Carnap have added elaborate theories of inductive logic to the literature, theories which in both cases embody the traditional calculus of probabilities.[2] In seeking to justify the use of their respective sampling-formulas they reject the view, shared by Williams, that a formula can give the sampler an odds-on advantage in any sampling situation. Instead, the formula is to be understood as stating a rule of procedure which, applied tirelessly over and over, will in the long run bring the sampler to a point at which theorems in the calculus of probabilities begin to guarantee him a pretty accurate reading.

This view, dwelling as it does upon the tendency of accumulated subclasses to converge at a true reading in the long run, might be tagged the eschatological defence of sampling. It is perhaps not without applications to some types of inductive reasoning, but it has no application at all to what the world calls sampling. The pride of a good sampling inference is that it stands solidly with us on the basis of statistically slight data, for its strength lies in what formulas as such must leave out : the sampler's alertness to his options in drawing a sample. Demonstrations of long-run efficacy clearly have no bearing upon the sampling of stuffs which, for reasons of haste or economy, must be judged at one shot, such as perishables and items soon to be moved through further processing or into market.

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[1] Ernest Nagel, Review of Donald Williams, "The Ground of Induction", *Journal of Philosophy*, xliv (1947), 652.

[2] See especially Hans Reichenbach, *Experience and Prediction* (University of Chicago Press, 1938), and *The Theory of Probability* (University of California Press, Berkeley, 1949). Also Rudolf Carnap, *op. cit.* and *The Continuum of Inductive Methods* (University of Chicago Press, 1951).

When we restore to our concept of sampling the normally accompanying language of ' how to go about it ', it becomes evident that the reliability (rather than probability) of sampling inferences springs from the watchful labours of the sampler, not from the application of a calculus which takes no account of those labours. If someone were to object that no pains short of nearly complete enumeration in the smallest degree force a sample *logically* to match a population, I should want to answer that the logical necessity he speaks of is in another world from sampling. It is a property built into certain kinds of inference-forms which are recommended for use when one is deducing or calculating but not, as it happens, when one is swimming, sailing, sweeping, or sampling.

If these remarks are correct, there is no problem of induction so far as sampling is concerned. Hume's problem applies only if we abide a mode of representation which deceives us into supposing that samplers depend upon an argument-form for their good results.

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IV.—ON MORAL DISAGREEMENTS

By Oliver A. Johnson

My subject in this paper is the well known difference of opinion between G. E. Moore and A. J. Ayer on the issue of whether people ever disagree with each other on moral questions. This dispute, besides being interesting in itself, is important for ethical theory because the real point at issue is the question of whether ethical knowledge is possible. It is Moore's belief that, by establishing that people do disagree on moral questions, he has proved the possibility of ethical knowledge hence refuted ethical scepticism while it is Ayer's belief that, by establishing that they do not do so, he has proved the impossibility of ethical knowledge hence demonstrated the truth of scepticism. My purpose in this paper is not to defend either of the disputants but rather to show that the arguments of neither are conclusive. In conclusion I shall suggest a different approach to the problem of ethical knowledge, one which I believe is more fruitful than that employed by Moore and Ayer.

In his defence of ethical scepticism, Ayer adopts a radically subjectivist interpretation of morals. So-called normative ethical judgments (*i.e.* judgments of ultimate right and wrong and of intrinsic good and evil), he argues, are not judgments at all. Rather they are simply expressions of the emotional attitudes of the person who makes them. As expressions of emotion they cannot possibly be true, hence they cannot convey knowledge. Moore, on the other hand, contends that such judgments can be true, hence can convey knowledge. At least some of these judgments, he believes, cannot be reduced to expressions of emotion; rather they are objectively valid. Therefore, ethical scepticism, which denies them any objective validity, is false. To substantiate their views both Moore and Ayer turn to the question of whether people ever disagree with each other on moral questions (questions containing normative ethical concepts). I shall commence with a brief statement of Moore's argument (which is in the form of a critique of subjectivism), then turn to Ayer's answer to Moore, then return to Moore for a fuller analysis of his argument.

Moore's argument against subjectivism runs as follows:[1] If it were true, as the subjectivists maintain, that all normative ethical assertions are simply statements describing the emotional attitudes of the person who makes them,[1] it must follow that no two people can ever have a difference of opinion on a moral question. Since each of the persons involved would be basing his stand purely on his own emotional attitude (whether he was aware of this or not), the opposition between them would not represent a difference in their opinions or beliefs about what is true in the realm of morals but would amount simply to a clash between their divergent subjective preferences. If, for example, the first contended that freedom is intrinsically good and the second denied this, their dispute would be of the same nature as if the first had said instead that he loved ice cream and the second had replied that he loathed it. In neither case would there be any difference of opinion between them, logically, since in both cases the speakers would simply be giving a report of their tastes in the matters of freedom and ice cream, respectively. But, Moore contends, men as a matter of fact *do* disagree about moral issues. At least sometimes their disputes go beyond the level of opposed personal preferences to that of contradictory opinions as to what is truly right or good. Because such differences of opinion do occur, the subjectivist theory, which implies that they are impossible, must be false.

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[1] See G. E. Moore, "The Nature of Moral Philosophy", in *Philosophical Studies* (London, 1922), pp. 333-334.

Ayer's answer to Moore's argument is simple. He readily agrees with Moore that, if the subjectivist theory is true, men cannot differ in opinion on moral questions. But he then asserts that, as a matter of fact, they never do. An examination of the actual data confirms, rather than refutes, subjectivism.[2]

Ayer's defence of subjectivism against Moore's criticism is open to two objections. (1) His contention that people never disagree with each other on moral questions is factually false, and (2) even if this contention were true, the conclusion which he draws from it is a *non sequitur*. I shall now try to justify these objections, commencing with the first.

Ayer's view that people never disagree about moral questions may, at first glance, seem too patently absurd even to require refutation. For, if they were never to argue such questions, mankind would be deprived of one of their most relished topics of conversation. The apparent absurdity of Ayer's view is quickly dissipated, though, when we recollect that by the term,

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[1] While Moore directs his attack against the broad subjectivist position that moral assertions are descriptions of the speaker's emotional attitudes, his criticism holds equally against Ayer's "radical" subjectivism, which interprets such assertions as direct expressions of those attitudes.

[2] A. J. Ayer, *Language, Truth and Logic*. 2nd. edn. (London, 1948), pp. 110-112.

moral questions, he means questions of ultimate right and wrong and intrinsic good and evil only. Ayer does not deny the existence of disputes in the realm of morals in general ; all that he denies is the existence of disputes on *moral questions*. That people do argue constantly about morals he recognises as well as anyone else ; he simply points out that these arguments, if examined carefully, will be found never to be concerned with ultimate right and wrong and intrinsic good and evil. Instead they will turn out to be disagreements concerning questions of fact (or of logic). If, for example, we get into an argument with someone about whether a certain action was right or wrong and attempt to convince him that he is mistaken in his moral judgment of that action, we always do so by pointing out that he is in error about the facts of the case involved ; that he has misconstrued the agent's motive or has failed to take into account all of the circumstances in which the agent was placed, etc. However, Ayer adds, if we come to the conclusion that our opponent is in complete agreement with us on the facts of the case and still disagrees with us in his judgment of the moral issue at stake, we give up arguing with him and resort to abuse (*cf. ibid.* p. 111).

While Ayer may be over-stating his case when he concludes that, if someone agrees with us on all the facts of a moral question yet disagrees with the moral judgment we make on it, we resort to abuse—philosophers, at least, should be able to rise above *that* level—it is difficult to deny his conclusion that we give up arguing the point. The crucial question is, Do we always do so ? Ayer thinks we do.

> If anyone doubts the accuracy of this account of moral disputes, let him try to construct even an imaginary argument on a question of value which does not reduce itself to an argument about a question of logic or about an empirical matter of fact. I am confident that he will not succeed in producing a single example (p. 112).

Since the question is a factual one, let us see if we can answer Ayer by constructing an imaginary argument of the type required.

G. " The one truth of ethics about which no one will dispute, I suppose, is the contention that pain is intrinsically bad."

W. " Just a minute ; I disagree. Pain is not either intrinsically bad. The only pain bad is that which is judged bad. If a person likes pain, his pain is good."

G. " I hadn't really expected anyone to disagree with me on this point but, since you have done so, I should reply that neither of the reasons that you have given in support of your disagreement can bear serious examination. For the question of whether a person judges a pain to be bad or not is irrelevant to its actual badness. And your assertion that a pain which a person likes is good involves an impossibility; such a feeling would be a pleasure, not a pain."

W. "I didn't realise that I had made two different points but I can see now that I did. Your reply to my second I can dispose of easily. If you *define* pain as a feeling which is disliked, it is obviously impossible for anyone to like pain. So I will shift my argument slightly and say that the statement 'Pain is intrinsically bad' is either a tautology or false. You are now left with two alternatives: Either the term 'bad' must be identical in meaning with the term 'disliked', in which case your 'truth of ethics' becomes 'feelings which are disliked are necessarily disliked', or else the two terms must differ in meaning, in which case I can say that it is possible for a person to dislike a feeling that he is experiencing and yet that that feeling may be good. Your second reply is more difficult to meet. I can see your point that the actual goodness or badness of something does not depend on our judgments about its goodness or badness. But what happens if we eliminate *all* judgment from the situation? What if all humans were wiped off the earth, leaving only animals—capable of feeling but incapable of forming judgments? I should say that in such a world nothing would be good or bad."

G. "In answer to your first point, I will simply slide between the horns of the dilemma you have offered me. 'Feelings which are disliked are intrinsically bad' is not a tautology; I can define 'disliked' without including the term 'bad' in my definition. Nevertheless, the statement is necessarily true. If a person dislikes a feeling he is experiencing, that feeling *must* be bad. How do I know this? The answer is that I intuit it to be true. It is an *a priori* truth of ethics. As for your second point, I think you are mistaken. I can imagine a world inhabited by animals, capable of feeling pain but not capable of judging that the feeling is bad. If all the animals in that world were suffering indescribable agony, I should say that evil just as surely exists there as it would exist in a world in which the sufferers were human beings. To make you agree with me on this point I ask you imaginatively to contemplate such a world as carefully as you can and then see if you still find yourself able to maintain the position you held earlier. . . ."

I am not concerned here with the validity of the arguments given in the dialogue above. The only point at issue is whether G and W have differed with each other on a *moral question*. It is indisputable, I should say, that they have. For the question

"Is pain intrinsically bad?" is clearly a normative ethical question. Not only have G and W differed with each other, but they have each presented arguments in defence of their views. While it is true that one of the arguments is concerned with a logical point, the remainder are ethical arguments. They give reasons either in support or denial of the proposition "Pain is intrinsically bad."

An examination of the history of ethics would turn up many such differences of opinion on moral questions. While I do not propose here to conduct a search into the literature to support this contention, I suggest that anyone who wishes to undertake such a project will find a good starting place in the writings of Ayer's opponent, Moore. *Principia Ethica*, for example, contains several such arguments.

Ayer's contention that people do not dispute moral questions is thus factually false. But—and this takes us to our second criticism of Ayer—even if his contention were true, the conclusion he draws from it does not necessarily follow. His argument is to the effect that, since people do not dispute about moral questions, their stands on such issues must be purely emotional, hence devoid of objective validity. The suppressed premise in this argument is the assumption that, for a proposition to be true, someone must deny it. A curious fact that Ayer has overlooked in his argument is that a lack of moral disagreements is equally as compatible with ethical objectivism as with subjectivism. If we assume that certain normative ethical propositions are true and that we can directly intuit their truth (as Moore and many of his intuitionist colleagues do), we remove the possibility of disagreements concerning them. For, if any two people are really intuiting the same ethical truth, they must of necessity be in agreement about it. Disagreements, when they occur, thus cannot be about the moral truth itself but about something confused with it—" questions of fact", etc. It is not my purpose in this paper to defend such an intuitionist theory. All I wish to show is that a lack of disagreement on moral questions can be explained as consistently in its terms as in terms of Ayer's subjectivism. Therefore, because he deduces his subjectivist position from an alleged lack of such disagreement without first eliminating this alternative explanation (as well as all other possibilities), his conclusion is a *non sequitur*.

So much for Ayer. Let us now return to Moore's argument. As we have already said, he maintains that, if subjectivism were true, people could not differ in opinion with each other on moral questions. As a matter of fact, however, they do; therefore subjectivism must be false. My objections to Moore's argument are either that it begs the question or else that it fails to be conclusive. Let us grant that we have shown, in answer to Ayer, that people *do* differ in opinion with each other on moral questions. Does it necessarily follow, therefore, that the *fact* of such differences of opinion implies an objective truth to be found in the subject-matter under discussion? Or may it not still be possible to reduce all such differences to opposed expressions of preference, or explain them in some other equally subjective way? In his argument Moore seems to give two different answers to these questions. He implies, in the first place, that if we could establish that differences of opinion on moral questions do in fact occur, then we should have demonstratively refuted the subjectivist position.

> [According to subjectivism] when *I* say "That was wrong" I am merely saying "That sort of action excites indignation in me, when I see it"; and when you say "No; it was not wrong" you are merely saying "It does not excite indignation in *me*, when *I* see it." And obviously both judgments may perfectly well be true together. . . . In other words, and this is what I want to insist on, if this view be true, then there is absolutely no such thing as a difference of opinion upon moral questions (p. 333).

Since people do differ in opinion on moral questions, subjectivism, because it make such differences of opinion impossible, must be false—*if* differences of opinion are the same as logical contradictions. For, however two expressions of emotion may be related to each other, they cannot logically contradict each other. But need differences of opinion be logical contradictions? This, of course, depends on what one means by the phrase "difference of opinion". If it be defined as "disagreement in which the opinions of the disputants are logically incompatible with each other", then obviously all differences of opinion are logical contradictions. By his assertion that subjectivism makes differences of opinion on moral questions impossible, Moore seems to be implying such an equivalence of meaning. In doing so, though, he begs the question at issue. For he rules subjectivism out of court by definition. To avoid Moore's conclusion, all that a subjectivist need do is to reject his definition. The subjectivist might agree that, while we should be justified in calling a discussion between two people a difference of opinion only if the assertions of each *verbally* contradicted those of the other, such an admission need not destroy his case. For the verbal form which the discussion actually takes may be misleading.

The assertions of the persons involved may still be reducible to expressions of attitude, parading under false colours (even though the persons themselves may not be aware of the fact). What the critic of subjectivism must demonstrate is that each disputant is in his assertion really attributing a predicate to a subject and that these predicates are logically incompatible with each other. And this he cannot do simply by pointing to the verbal form which the assertions take.

To defend his definition of "differences of opinion" Moore would have to show that the explanation offered by the subjectivist is self-contradictory. Could he do this ? Obviously not. While the subjectivist interpretation of differences of opinion may not in fact be true, it cannot be ruled out *a priori*. For it is certainly at least possible that, when we believe ourselves to be discussing moral questions objectively and dispassionately, we are really fooling ourselves. Hence, Moore's first argument, because it rules this possibility out by definition, begs the question.

Moore seems himself to be aware that the mere occurrence of moral disputes does not provide a logically conclusive argument against subjectivism. For in the sentence following those quoted above he writes : "If two persons think they differ in opinion on a moral question (and it certainly seems as if they sometimes *think* so), they are always, on this view [subjectivism], making a mistake, and a mistake so gross that it seems hardly possible that they should make it : a mistake as gross as that which would be involved in thinking that when you say 'I did not come from Cambridge to-day' you are denying what I say when I say 'I did'" (*ibid.* pp. 333-334). This statement is clearly inconsistent with the one which precedes it. If it follows from the subjectivist view that there is *absolutely* no such thing as a difference of opinion on moral questions, then, whenever people think they are so differing, they are *necessarily* making a mistake. Rather than being a mistake it is hardly possible to make, the mistake is logically unavoidable.

Recognising the shift that Moore makes in his case, I think we should agree that his second position represents the stronger argument against subjectivism. But just how strong is his argument ? What he is implying is that subjectivism, by ruling out the possibility that people can discuss moral questions dispassionately—that is to say, with their purpose being to discover the truth in the matter under discussion—is false to the facts. Although he would no doubt admit that many, and perhaps most, arguments that people get into over moral questions are simply verbalisations of conflicting subjective attitudes, he would deny that all are. For at least sometimes some people do succeed in examining the issues on their own merits, refusing to allow their own emotional or other peculiarities to colour their thinking in any way. If we approach Moore's argument in the same spirit of objectivity that he believes it possible for us to employ in deciding moral questions, I believe that we must grant him to have a strong case. Anyone who has studied the history of moral philosophy will, I think, find it difficult to deny Moore's conclusion. Many examples could be given in defence of this judgment ; a couple must suffice here. To anyone acquainted with the ethical writings of Henry Sidgwick, the suggestion that he reached his conclusions in any other way than through a dispassionate evaluation of the arguments and evidence must seem well-nigh incredible. For his *Methods of Ethics* is a model of objectivity and impartiality. Rather than engaging in special pleading on behalf of his own views, Sidgwick invariably bent over backwards to be fair to opposed positions. The same kind of open- and fair-mindedness is characteristic of the ethical writings of Moore. This is particularly true of his critique of Sidgwick, as it appears in Chapter III of *Principia Ethica*.

Moore's case against the subjectivist interpretation of normative ethical assertions based on an appeal to the facts could be expanded indefinitely. But we must turn to the other side and ask : How would a subjectivist reply to this evidence ? What he would have to do would be to maintain that the evidence has in some way been misinterpreted, that it does not support the conclusion which his opponents derive from it. How could such a contention be defended ? A century ago the case would probably have seemed hopeless. Not so, though, today. For we have come to realise, through research in anthropology, sociology, psychology, and related fields that we are not the rational beings we have led ourselves to believe that we are. On the contrary, we must, if we are candid, admit that much of what we label rationality is really rationalisation. And nowhere—except, possibly, in religion—is this admission more true than in morals. Hence, when Moore contends (as he does in the last quotation above) that, if subjectivism were true, whenever people think they are having a difference of opinion on a moral question, they must be making a mistake as gross as they would be if they thought they were disagreeing when one said " I did not come from Cambridge today " and the other said " I did ", he has greatly overstated his case. Discussions about matters of fact and discussions about right and wrong or good and evil are simply not analogous. For the latter are constantly subjected to subversion and distortion by subjective factors, both conscious and unconscious, which the former can usually escape.

The question here at issue is one which can be argued endlessly —and has. The point that I wish to make in this paper is that none of these arguments can ever be logically conclusive. However ingenious their methods of explaining away apparently objective discussions of moral questions, the subjectivists cannot demonstrate the non-existence of such discussions. And however overwhelming the evidence they accumulate in favour of the existence of such discussions, the opponents of subjectivism cannot produce a single example which they can demonstratively show to be inexplicable in subjectivist terms. If one takes a stand on this issue, therefore, he must do so on grounds which fall short of being conclusive. The same conclusion holds true, furthermore, of the entire dispute between Moore and Ayer. The truth or falsity of ethical scepticism cannot be demonstrated by an inquiry into the question of whether people do or do not in fact differ in opinion from each other on moral questions. While Ayer is mistaken in his contention that such differences of opinion do not in fact occur, Moore is unable to show that their occurrence is incompatible with the truth of ethical scepticism.

Having reached this point, the proper conclusion to draw would seem to be that, since Ayer and Moore cannot both be correct and since neither can demonstrate the truth of his own view, we must accept one or the other on the basis of reasons that are less than logically coercive. Such a response would be appropriate, *if* Moore's and Ayer's approach to the problem were the only possible one. I do not think it is. In conclusion I should like briefly to suggest a method of approach which, if successful, could provide a demonstrative answer to the question —Is ethical scepticism true ?

The approach which I should like to suggest is one derived, in part, from the methodology usually employed by Ayer, a methodology which, unfortunately, he abandoned in his attempt to find an answer to Moore. Ayer's denial of the existence of disagreements on moral questions, besides being false, was, from his own point of view, unnecessary. For he already had, in the verification principle, a proposition which, if itself defensible, could serve as a premise from which ethical scepticism could be deduced. Granted the verification principle, ethical scepticism necessarily follows.

It seems to me that Ayer's general methodological approach to the problem of ethical knowledge is sound. By this I mean that, if we wish to find a conclusive answer to the question, Is ethical knowledge possible ? we should not concentrate our attention directly on ethical assertions at all. Rather, we should attempt to discover a proposition, or set of propositions, whose truth can be demonstrated, then show that the logical relationship between it and normative ethical propositions is such as either to entail or to contradict ethical scepticism. This is what the positivists attempt to do with the verification principle. The trouble with their whole position, though, is that the verification principle, rather than being demonstratively true, is itself self-contradictory. And though the positivists have tried in a variety of ways to patch the verification principle up, these efforts have ended in failure. It is clear to philosophers by now—and this is true even of most positivists—that the verification principle must be abandoned. Hence no conclusions can be deduced from it, either pro or con, concerning the truth of ethical scepticism.

But there are other propositions besides the verification principle which may well be logically related to normative ethics. The kind of proposition we need is one which is itself demonstratively true. If we could then establish a logical relationship between it and the ethical propositions in question, provided this relationship was one either of entailment or of inconsistency, we should have an answer to the question of whether ethical knowledge is possible.

I am not sure that such a programme could be successfully completed. But I believe it may well be possible. For the demonstratively true proposition with which such an argument might begin I suggest some variant of the following : " Some (non-tautologous) propositions are true." Since ethical scepticism is the denial of ethical knowledge, its thesis can be stated in the proposition : " No normative ethical propositions are true." The problem then is this : What is the logical relationship between the two propositions above ? If it can be shown that the first is true and that it entails the second, then ethical scepticism can be established. If, as I am inclined to believe, it can be shown that the two are logically contradictory, then ethical scepticism can be refuted.

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V.—UNCHARACTERISTIC ACTIONS

By Betty Powell

Of any given man, we might say that he is ambitious, proud, generous or mean, vain or modest, honest or dishonest; that he has a generous disposition or has a great deal of patience. To say any of these things is to say something about his character. But what is 'his character', and how are his character and his actions connected ?

Libertarians and Determinists have considered the question of whether, if a man's actions are predictable from a knowledge of his character, he is then free, and Libertarians have maintained :

(*a*) that some actions are not predictable from a knowledge of the agent's character,
(*b*) that these actions are free actions, and
(*c*) that these actions are peculiarly moral actions.

I want to defend the thesis that some actions are not predictable from a knowledge of the agent's character, that is, that men do, sometimes, act uncharacteristically.

In defending the possibility of uncharacteristic actions, Libertarians see the importance of this for responsibility and morality. If a man reforms, there must be some point at which he does not act characteristically. Libertarians see that in refusing to allow that actions can be performed which are not predictable from our knowledge of the agent's character, we fail to allow for the possibility of a man's becoming, for example, generous. But, although they realise the importance of uncharacteristic actions, their accounts of what uncharacteristic action is, seem to me to be unsatisfactory. I do not wish, therefore, to defend the view that only uncharacteristic actions are 'free' actions, whatever a 'free' action is, or that uncharacteristic actions are peculiarly moral actions. Although I shall be concerned with uncharacteristic actions in specifically moral contexts, there is no reason to suppose that only moral as opposed to morally neutral actions are uncharacteristic, and certainly no reason to suppose that only uncharacteristic actions have moral worth. In fact, the possibility of uncharacteristic actions is more easily shown by referring to those which are morally reprehensible.

Both Libertarians and Determinists have tended to talk of character as some sort of force, which must be arrested before a man can perform an action which is not predictable from a knowledge of his character, as something which causes a man to act as he does. A man's actions are in some sense a result of his character ; his actions ' flow ' from his character, although Libertarians claim that on some occasions an action may not ' flow ' from the agent's character. For example, Professor Campbell says :

> . . . in the situation of moral conflict, I, as agent, have before my mind a course of action, X, which I believe to be my duty ; and also a course of action, Y, incompatible with X, which I feel to be that which I most strongly desire. Y is, as it is sometimes expressed, ' in the line of least resistance ' for me—the course which I am aware that I should take, if I let my desiring nature operate without hindrance. It is the course towards which I am aware that my *character*, as so far formed, naturally inclines me. . . . I cannot help believing that it lies with me, here and now, quite absolutely, which of two genuinely open possibilities I adopt ; whether, that is, I make the effort of will and choose X or, on the other hand, let my desiring nature, my character as so far formed, ' have its way ', and choose Y, the course in the line of least resistance (MIND, 1952).

A criticism of the Libertarian position which Campbell feels obliged to counter, is what he calls the ' Caprice ' criticism. In *Scepticism and Construction* he says :

> The essence of the Caprice criticism can be stated quite shortly and simply. ' The Libertarian alleges ', we are told, ' a type of willing whose peculiarity is that it is not, or not completely, determined by the character of the agent (in relation to his circumstances'). But in what sense is an act which does not issue from the character of the agent the agent's *own* act ? Surely it cannot be understood as *his* act at all. It must rather be ascribed to the intervention of some external agency.

A dilemma, which he thinks is a false dilemma, is forced upon the Libertarians by those who employ this criticism.

> *Either* you have intelligible continuity of act with character, they urge, *or* you have what, so far as the agent is concerned, is mere ' chance '.

In answer to this criticism, Campbell says :

> We are not compelled to think of ' chance ' as the sole alternative to complete causal continuity between character and act. We *can* attach a meaning to an act which is not causally continuous with our character and is yet our own act if we look for the meaning in the right place, *viz.* in the actual experience of willing (pp. 160-162).

The essence of the Caprice criticism, and the Determinist position seems to be that there are no uncharacteristic actions ; a man cannot act uncharacteristically, for if an action does not issue from the agent's character, it is not 'his'. The essence of Campbell's account is that an uncharacteristic action is one that is *not* causally continuous with a man's character, although a characteristic action *is* causally continuous with his character, but that uncharacteristic actions need not be considered as chance actions.

Now it has been pointed out, both by Professor Ryle and Professor Nowell-Smith, that character is neither a force nor a cause. But it is not enough simply to say this. Neither Ryle nor Nowell-Smith allow for uncharacteristic actions, and Nowell-Smith uses a form of the Caprice criticism, even though this seems to be more apt in a causal account of character.

Nowell-Smith, in his book *Ethics*, discussing the Libertarian theory as expounded by Campbell, says that the essence of Campbell's account is that a 'free' action should not be predictable from a knowledge of the agent's character. "But", he says, "if this is so, can what he does be called 'his' action at all ?" Leaving aside the question of whether or not such an action is 'free', it seems that, according to Nowell-Smith, and on a non-causal account of character, an action which is not predictable from a knowledge of the agent's character, is not to be called 'his' action at all. If this is to have bearing on the question of responsibility, as it is presumably intended to have, it seems to mean that we should not assign credit or blame, hold the 'agent' responsible for such an action, *i.e.* one which is not predictable from a knowledge of his character. Nowell-Smith goes on to say :

> Is it not rather a *lusus naturae,* an Act of God or a miracle ? If a hardened criminal, bent on robbing the poor-box, suddenly and *inexplicably* fails to do so, we should not say that he *chose* to resist or deserves *credit* for resisting the temptation ; we should say, if we were religious, that he was the recipient of a sudden outpouring of Divine Grace or, if we were irreligious, that his 'action' was due to chance, which is another way of saying that it was inexplicable. In either case we should refuse to use the active voice (pp. 281-282).

What is undoubtedly asserted here, is that if the action is not predictable from a knowledge of the agent's character, it is not one for which we should give him credit.

The question of whether an uncharacteristic action is possible is to be distinguished from that of what it is to perform an uncharacteristic action. Campbell's account of uncharacteristic action is open to serious criticism, but it is by no means the case that because his analysis is unsatisfactory, there is nothing which requires analysis. Nowell-Smith seems to maintain that uncharacteristic action is not possible. He rightly objects to the way in which Campbell talks of character, as if it were some sort of force which restricts a man's freedom, as if characteristic actions were actions caused by a man's character.

> It is noticeable that, on Campbell's analysis, a man's desires and even his character are continually referred to as ' it ' ; desires are thought of as forces which, sometimes successfully and sometimes unsuccessfully, prod a man into doing what he ought not, and his ' character as so far formed ' is the sum of these forces.

But Nowell-Smith's account of what a characteristic action is, is unenlightening. What I do, he says, will depend on my character, and this is not to say that my choice is restricted. " For to say that my choice depends on my character is not to say that my character compels me to do what I do, but to say that the choice was characteristic of me " (pp. 287-288).

This denial that character is a force or a sum of forces, or any sort of cause, throws no light on the questions of what an uncharacteristic action is, or whether such action is possible. If there is no such thing as uncharacteristic action or choice, what is the point of saying that certain actions or choices are characteristic of me ? It would seem that on Nowell-Smith's account, one cannot perform an uncharacteristic action, for if one did perform an action which was not predictable from a knowledge of one's character, what one ' did ' would not be accounted ' one's action '. We should refuse to use the active voice.

We do, as a matter of fact, know of a great many uncharacteristic actions. We often say, for example, ' I should never have believed that of him ', ' I can't believe that he would do a thing like that ', and so on. But when a highly respected and respectable solicitor pleads guilty to embezzling his clients' money, we might *say* that we should never have believed it, but there it is. It should be sufficient, I think, to show that there is something suspect about the notion of an action's not being ' his ' unless it is predictable from a knowledge of the agent's character, to point to first offenders. Admittedly, not all first offenders are people who act uncharacteristically. Of some first offenders we have said that they will be in serious trouble sooner or later, but some of them do surprise us, some of them do act uncharacteristically.

If we were to take Nowell-Smith seriously, it would seem that we ought not to blame the man who embezzles his clients' money, on the grounds that, since this action was not predictable from a knowledge of his character, it was therefore 'not his'. It is certainly true that in some cases of uncharacteristic actions the agents are dealt with leniently. A man who has never stolen anything before may be treated leniently because this is his first offence. But it is still an offence and it is still his. I do not wish to deny that men sometimes perform actions for which we say that they are not responsible, and for which we would not assign credit or blame. A man may not be held responsible for an action which he did not know he was doing, a defence often offered on behalf of the man accused of murder. I suppose this might be put by saying that in these cases the action was not really 'his'. But we do not refuse to assign blame to the murderer because this is his first murder and could not have been predicted from a knowledge of his character. We may refuse to assign credit or blame to certain actions a man performs, but this is not because they are not predictable from a knowledge of his character.

Libertarians seem to see, in the denial of the possibility of uncharacteristic actions, the denial of the possibility of, for example, an ungenerous man's becoming generous, or an ungenerous man's performing a generous action. Few people, however, would deny that an ungenerous man may become generous, although they may wish to deny that an ungenerous man may act generously, that he may act uncharacteristically. Those who maintain the Determinist point of view that a man's actions, unless they are not to be accounted 'his', must be predictable from a knowledge of the agent's character, try to allow for an ungenerous man's becoming generous. His character is considered as something that is amenable to change. It is not impossible for a man to change his character, although it may be difficult. Nowell-Smith says: "Men would not employ a special form of speech for changing the character and conduct of others unless they had a pro-attitude towards those changes" (*Ethics*, p. 301). Ryle talks in much the same way: "... the relative strengths of inclinations are alterable. Changes of environment, companionship ... can all modify the balance of power between the inclinations which constitute one side of a person's character. But so can his own concern about this balance modify it" (*The Concept of Mind*, p. 112). Traits of character, or dispositions, are not completely unchangeable, but they are difficult to change. They are not absolutely permanent, but they are more or less permanent. It is not impossible to change your character, but you have to try hard, or other people have, to change it. If you are an ungenerous, or an honest man, you may indeed act generously or dishonestly, but you have to change your character first, for then your action will be such that it can be predicted from a knowledge of your character, or explained by reference to your character. There are others, of course, who wish to maintain that whatever a man's character is, he can still act generously; no matter how honest a man is, he can still act dishonestly, *i.e.* without changing his character. Whatever a man's character is, he can still act uncharacteristically. For example, J. D. Mabbott says: " I remain convinced that moral responsibility requires that a man should be able to choose alternative actions, everything in the universe prior to the act, including his self, being the same." [1]

It seems to me that there are uncharacteristic actions, or at least, actions which we recognise as uncharacteristic when we are not engaged in philosophising. What is required is an account of the relations between a man's character and his actions which will allow us to admit uncharacteristic as well as characteristic actions. What Mabbott, for example, seems to require is that we should be able to assert that although Smith is dishonest, he did act honestly on this occasion. I shall offer an account of the relations between statements about a man's character and statements about a man's actions: between ' Smith is a generous man ' and ' Smith acted generously ' which I hope will allow for this. The question turns on what it involves to say that Smith is a generous man, and what it involves to say that Smith acted generously on this occasion. First, however, we need to ask what it is to say that *a man* is generous, or that *a man* acted generously, *i.e.* what ' being generous ' and ' acting generously ' mean.

To say that a man is generous, is not to say that he is doing anything now, for we may say of a sleeping man that he is generous, or mean, or honest, or dishonest. There is no action which is denoted by saying that a man is generous. Instead, there is a whole range of actions to which the word ' generous ' will apply. For example, the range of actions to which the word ' generous ' is applicable includes giving money to this or that charity, giving money or other help to the poor, providing hospitality, and so on. ' Honest ' is also applicable to a range of actions, *e.g.* speaking the truth, giving the correct change if you are a shopkeeper, to use one of Kant's examples. 'Dishonest' includes cheating, stealing, lying and the like.

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[1] " Freewill and Punishment ", p. 289, *Contemporary British Philosophy, Third Series*, ed. H. D. Lewis.

There are certain sorts of actions which are considered generous, mean, honest, dishonest. The actions which can be called 'generous' are not completely specifiable; the meaning of 'generous' cannot be given completely by recounting the actions which count as generous ones, for there is always the possibility of new actions being included; new charities, new causes, new means of helping those in need. 'Helping those in need' can itself be broken down, although not completely, into more specific acts.

There is also a point at which we might no longer be prepared to call an action 'generous', but perhaps, for example, foolish, if the giving of a large sum of money to charity were to reduce the donor and his family to poverty; we might not be prepared to call certain acts of speaking the truth 'honest' but 'tactless'. Although we may disagree about the applicability of these terms to certain actions, there is a broad measure of agreement about the sorts of actions which do qualify for these terms. We may disagree about whether or not the giving of sixpence on a flag day is a generous action, but we should agree that the taking of sixpence out of the box is not a generous act.

The meaning of terms like 'generous' and 'honest' may also be unpacked, to use Ryle's term, in a series of statements, categorical or hypothetical, about a generous man. The range of application of terms like 'generous' may be shown by outlining the things a generous man does, or the things a generous man would do. For example:

> To say that a man is generous is to say that he gives to deserving causes, gives food to beggars, helps those in need, etc. A generous man is one who. . . . To say that a man is generous is to say that he will. . . .

No doubt a case might be made for regarding the hypothetical method as better here, partly because we cannot specify all the acts which count, or might at some future time count, as 'generous', and partly because we might want to insert qualifications, *e.g.* 'To say that a man is generous is to say that if he is aware of someone's need he will try to alleviate it . . . etc.'. We explain the meaning of the term 'generous' by outlining, as far as we can, the range of actions to which the term is applicable. Actions of this, that and the other sort, are generous ones. A generous man is one who performs actions of this sort.

There are now the further questions of whether this particular man Smith acted generously on this occasion, and whether this man Smith is generous; the questions of whether 'generous' applies to his action, and whether 'generous' applies to him. The range of application of the term 'generous' provides us with a criterion by which we can decide whether or not Smith acted generously on this occasion. If Smith took sixpence out of the collecting box, we have no hesitation in deciding that this was not a generous action, for taking sixpences out of boxes on flag days is not one of the things which, by definition, a generous man would do. It is not one of the actions to which the word 'generous' is applicable. It is necessary, if his action is to be called a 'generous' one, that it should be one of the actions to which the word 'generous' is applicable, but this is not sufficient. Smith must also have known what he was doing, for we do not consider that a man acts generously by accident, and we should not say that he had acted generously if, for example, he gave away money in his sleep. Briefly, a man knows what he is doing (*a*) if he could say, if he were asked, what he is doing at the time at which he is doing it, and (*b*) if he could give his reason, if he were asked, for what he is doing, at the time at which he is doing it.

Further, even though a man does perform one of the actions to which the word 'generous' is applicable, knowing what he is doing, this is still not sufficient, for we need also to take into account his reason for doing what he does, *i.e.* his motive. Even though Smith did give a large sum of money to a deserving charity, and even though this is one of the actions to which the word 'generous' applies, we should refrain from saying that on this occasion he acted generously, if, for example, his reason for giving the large sum was to gain publicity, and if he would not have given so much if he had not thought that he would gain publicity. Kant would refuse to say that a shopkeeper acted honestly, even though he did give the correct change, if his reason for doing so was simply that this would increase his trade. There are, however, other terms, the sufficient condition for the application of which is simply that the agent should have performed one of the acts of a certain sort, knowing what he was doing, that is, where no account is taken of motive. We do not consider the motive of the man who runs off with the organisation's funds before we decide whether or not he acted dishonestly. It is sufficient that he knew what he was doing, and that this is one of the acts to which the word 'dishonest' applies. Although in the case of the shopkeeper's acting honestly, we might have no difficulty in outlining the criteria for saying that he acted honestly, there may be difficulty in knowing that the criteria have been satisfied, in knowing that the shopkeeper was not concerned with furthering his trade. For we have to ask him what his motive was, and he may lie to us. This is not to say that the criteria are such that whether or not they are satisfied is in principle unknowable. The shopkeeper knows what his reason was, and he, if no-one else, can say whether or not the criteria have been satisfied.

However, as I do not think that my argument rests on what criteria are to be satisfied, but simply on the fact that there are criteria, I shall assume that the necessary and sufficient conditions for saying that Smith acted generously are that he should have performed an action to which the word 'generous' is applicable, and that he should have known what he was doing, and in what follows, I shall take the latter for granted. The inclusion of a requirement with regard to the agent's reason among the criteria makes it more difficult for those other than the agent to know that the criteria have been satisfied, but although this is not unimportant, the argument does not rest on it, and in view of controversies about motives, it will be better, for the moment, to ignore it.

So, a generous action is one which, by definition, a generous man would do. It is one to which we are prepared to apply the word 'generous'. Smith acts generously each time he performs an act of this sort, if action regardless of motive is sufficient; otherwise he acts generously as often as his action fulfils the criteria. For Smith himself to be said to 'be generous', apart from its being said that he acted generously on this occasion, he must have performed a reasonable, not too accurately specifiable, number of actions which are generous ones. If he has only once or twice acted generously he cannot be said to be generous. If he acts generously on a reasonable number of occasions, he may justifiably be said to be a generous man. The statement 'Smith is a generous man' can be justified by referring to the occasions on which Smith acted generously. Two things are to be noticed about this:

(1) The statement 'Smith is a generous man' is dependent on the occasions on which Smith has acted generously.

(2) To say truthfully 'Smith acted generously', on this occasion, we need to know the criteria for acting generously, given, in part at least, in the meaning of the term 'generous', and to know that the criteria have been satisfied.

If 'Smith is a generous man' is a statement about Smith's character, there is no reason why we should not allow that Smith may sometimes act uncharacteristically. To say that Smith is a generous man does not involve saying that it is not possible for Smith to perform an action which does not satisfy the criteria for calling his action 'generous', for each of Smith's actions, no matter what we have already established about his character, or whether we have established anything about his character, must satisfy certain criteria before they are said to be generous and before Smith can be said to have acted generously; and the criteria in each case are independent of what we have established, if we have established anything, about him. So there is no necessity for saying that before Smith can act generously, if he is not a generous man, he must change his character.

The difference between the account of the use of words like 'generous' given above, and the accounts which demand that a man must change his character, if he is ungenerous, before he can act generously, is this: in the above account, 'Smith acted generously', on this occasion, is independent of the statement 'Smith is generous', although 'Smith is generous' is dependent on the occasions on which we might truthfully say 'Smith acted generously'. On the other account, 'Smith acted generously' is taken to be dependent on 'Smith is generous'.

The accounts which result in the necessity for changing one's character are those which consider actions as 'proceeding' from the agent's character; as exercises of his generous disposition; as a result of his character; as causally continuous with his character; as dependent on his character (Nowell-Smith: "What I do will depend on my character"), or regard the character of the agent as accounting for the way in which he acts (Ryle: "When we ask 'Why did someone act in a certain way?' this question might, so far as its language goes, either be an inquiry into the cause of his acting in that way, or be an inquiry into the character of the agent which accounts for his having acted in that way on that occasion. I suggest . . . that explanations by motives are explanations of the second type and not of the first type." *The Concept of Mind*, p. 89). All these accounts, in some sense, suppose that character statements like 'Smith is generous' are primary and that they explain or account for actions, instead of supposing, as outlined above, that statements about actions are primary and explain statements about character.

It seems to me that what they in fact do is to confuse statements like 'Smith is generous', with those expounding the meaning of the term, *e.g.* 'A generous man will . . . if . . .'. Whether or not Smith's actions are to be considered generous depends on what sorts of acts, performed by any man, are to be considered generous. Whether or not Smith's actions are generous ones will depend on what actions may fill in the gaps in 'A generous man will . . .'; whether Smith's actions are generous will depend on what we mean by 'generous'. But whether or not Smith's action is generous does not depend on what we have discovered about Smith's character, or on the statement that Smith is a generous man.

It seems to me that Ryle is guilty of this confusion. His account of what he calls 'motive explanations' seems to suggest that a man cannot act generously unless he is a generous man. He regards a character statement like 'Smith is generous' as prior to 'Smith acted generously', as one which explains Smith's actions. He says:

(*a*) that statements like 'Smith is generous' are established from instances—Smith's actions and reactions.

(*b*) that such statements can be used to explain subsequent actions, and

(*c*) that such statements function as inference tickets.

Let us dispose of the last point first. I do not wish to deny that such statements function as inference tickets, but to emphasise that they do not do so alone, but only in conjunction with our understanding of the meaning of 'generous'. If we say of Smith that he is generous, the person to whom we say this is entitled to expect that Smith will do certain, not too accurately specifiable things, if certain circumstances arise. If one is told that a particular man is generous, one has some idea of the sorts of things he has done and can be expected to do because one knows, roughly, the range of actions to which the word 'generous' is applicable. Why else do prospective employers ask for accounts of people's characters if not to gain some idea of what to expect from them ? This is not of course, to say that misunderstandings may not arise. One person might be prepared to apply the word 'generous' to actions to which another person might not. And it is not to say that one's expectations might not be unfulfilled. An employer, who is told that Smith is honest, is entitled to expect that Smith is to be trusted with the petty cash, because an honest man, by definition, is one who can be trusted with the petty cash. But it does not follow that his expectations will be fulfilled, that Smith will never pilfer the petty cash.

When we have experience of a man's performing some actions of a certain sort, *e.g.* generous ones, we have reasonable grounds for expecting that he will perform others of the same sort, *i.e.* others actions which are also generous ones. If Smith has never been in a situation in which he has been in charge of money, but has been found to be honest in other respects, we are entitled to expect that he can be trusted with the charge of money. If we have no knowledge of the ways in which Smith acts in certain sorts of situations, we have no grounds for expecting him to behave in certain other ways. If, however well we know Smith, we have never been in a position to know, for example, whether or not he arrives on time for appointments or for work, etc., we are not entitled to form any expectations about whether he will or will not be on time on this occasion.

It seems at first sight that there is no difference between Ryle's account of the way in which we establish character statements like ' Smith is generous ' and that given above. In *The Concept of Mind* he says :

> The ascertainment of a person's mental capacities and propensities is an inductive process, an induction to law-like propositions from observed actions and reactions. Having ascertained these long-term qualities, we explain a particular action or reaction by applying the result of such an induction to the new specimen, save where open avowals let us know the explanation without research (p. 172).
>
> In fact, however, we do discover the motives of other people. . . . It is or is like an inductive process, which results in the applications of them as ' reasons ' for particular actions. What is established in each case is or includes a general hypothetical proposition of a certain sort. The imputation of a motive for a particular action is not a causal inference to an unwitnessed event, but the subsumption of an episode proposition under a law-like proposition (p. 90).

It is tempting to assume that it is what the agent has done in the past which justifies our establishing the law-like proposition ' Smith is generous ' ; that we say that Smith has acted generously on this, that and the other occasion, and so we are justified in saying that Smith is a generous man, and this would be exactly the same as the account given above. Unfortunately, Ryle does not, as far as I know, give examples of the sorts of actions from which we establish these propositions, although he does give examples of those which are to be considered as instances of the observances of the law-like proposition, *e.g.* " The statement ' he boasted from vanity ' . . . is to be construed as saying ' he boasted on meeting the stranger and his doing so satisfies the law-like proposition that whenever he finds a chance of securing the admiration and envy of others, he does whatever he thinks will produce this admiration and envy ' " (*The Concept of Mind*, p. 89).

Are these actions from which we make our inductions generous actions, or are they actions which are morally neutral ? We cannot establish that Smith is a generous man by observing the occasions on which he acts generously, for Ryle does not allow for the possibility of a man's acting generously on a particular occasion unless he is a generous man. Again and again, Ryle insists that motive words, *e.g.* ' generous ', ' vain ', are dispositional words, and in so far as he insists on this, it seems that there are no actions which may be described as ' acting generously ' or as ' generous actions ' for this would be to use ' generous ' non-dispositionally. Generosity is a quality of a man, and ' generous ' applies to a man. There are of course, acts which may be described as exercises of his generosity, his disposition, by analogy with the acts which may be described as exercises of his knowledge of French. A man's acts can be described as generous if he is a generous man. The puzzle is to find the way in which we discover that Smith is generous.

If the statement ' Smith is generous ' is established by observing the occasions on which Smith acted generously, we require an account of ' acting generously ' which is independent of Smith's character, and if we have such an account, we do not require our knowledge of Smith's character to explain his subsequent action, for his subsequent action could also be accounted generous, independently of his character. Ryle maintains that we apply the result of our induction to the new specimen ; that we use these statements to explain subsequent actions.

It is surely incorrect to say that a word like ' generous ' is applicable to this action by virtue of what the agent has done in the past, whatever sorts of actions and reactions this may involve. We do not want to say, for example, when Smith refuses to give a donation to a deserving cause, that this is a generous action, because Smith has always acted generously in the past ; because we have established that he is a generous man. The man who runs off with the funds cannot be said to be acting honestly even though he has always acted honestly in the past. To take a more obvious example : a man might always have been late in the past, giving you just cause to say of him that he is unpunctual, in a dispositional sense, and giving you just cause to expect that he will be late on this occasion. But we do not want to say that he is late on this occasion because he has always been late before, particularly if on this occasion he arrives ten minutes before and not ten minutes after the appointed time. If, on the other hand, he is late on this occasion, we may explain this by saying that he is unpunctual. That is, we might have established the law-like proposition to the effect that whenever he is asked to be at a certain place at a certain time, he will arrive ten minutes or more after that time, but only the occasions on which he does arrive ten minutes or more after the appointed time can be regarded as instances of the observances of the law-like proposition. We need, that is, some criterion for deciding that he is late on this occasion, which is independent of his character or his past actions, before we can say that this action satisfies the law-like proposition that whenever. . . . The notion that a man's character accounts for his actions, or that his actions depend on his character, is plausible only in so far as the actions with which we are concerned are *e.g.* generous, that is, only in so far as they are generous actions can they be accounted for by 'He is generous'. And this very plausibility obscures the fact that we need some criterion for deciding that Smith acted generously on this occasion, before we can claim that this action is explicable by 'Smith is generous'. Smith might, after all, be generous and honest. We need some criterion for deciding whether this action is generous or honest before we can say which of the law-like propositions we have established about Smith is satisfied by this particular action.

It is the failure to recognise the distinction between a statement like 'Smith is generous' and one expounding the meaning of the term 'generous', *e.g.* 'To say that a man is generous is to say that he will . . . if . . .', which accounts for the requirement that before a generous man can act ungenerously he must change his character. A generous man cannot, by definition, perform acts like robbing the poor box. Smith, who is a generous man, very well might.

The requirement that the generous man should change his character before he can perform an ungenerous act, is a result both of assuming that 'generous' refers to some more or less lasting trait in his character, which causes him to act as he does, and which must be weakened or eradicated before he can perform an ungenerous action, and of confusing sentences which give the definition of a word, and sentences which use the word of a particular man. It is useless to deny that character is a cause, and yet to continue to accept the logical implications of that view. We understand well enough what it is for a generous man to perform a generous action, but how can an ungenerous man perform generous actions without changing his character? Of course a generous man does generous things, and the sorts of things he does can, to some extent be specified if we unpack 'generous'. When it is maintained that to say that a man is generous is to say that he gives to charity, or that whenever he is asked for financial help he will give it, etc., the meaning of the word ‘ generous ’ is being given. To ask what sort of actions a generous man performs is to ask what sort of actions are considered to be generous. The relation between a generous man and a generous man's actions is a quasi-analytic one ; to talk of a generous man's robbing the poor savours of contradiction. A generous man cannot, logically, act ungenerously, an honest man cannot steal, and a modest man cannot boast. The words ‘ generous ’, ‘ honest ’, ‘ modest ’ do not apply to actions of this sort. A generous man could, logically, rob the poor if we included ‘ robbing the poor ’ among the actions to which the word ‘ generous ’ will apply. But to say in this sense that a generous man cannot act in any way other than generously is not to say that it is impossible or even difficult for a particular man, Smith, to act uncharacteristically. If Smith is generous, there need be no question of his changing his character before he acts ungenerously, although there may be one of deciding whether or not the word ‘ generous ’ will continue to be applicable to Smith. To act ungenerously need not be difficult, even if you have always acted generously, and even if you are said, with justification, to be a generous man, and it is certainly not impossible unless you change your character.

Consider the fable of the boy who cried ‘ Wolf ’. No doubt the people who had on more than one occasion rushed to his aid only to find that there had been no wolf, called him a liar. They called him a liar because of the occasions on which he had lied. They were justified, because of what he had done, in expecting him to lie on subsequent occasions, and in expecting that he was lying on this occasion. But on this last occasion, their expectations were unfulfilled. The boy did tell the truth. On this occasion, in calling ‘ Wolf ’, the boy was calling truthfully, and it can be said that the boy acted uncharacteristically. There is no reason to suppose that before he called truthfully he had to change his character, or to suppose that he found calling truthfully at all difficult, even though a liar, by definition, is a man who does not speak the truth.

The criterion then, for deciding that Smith's action is generous, mean, honest or dishonest, is *never* his character. The statement ‘ Smith is generous ’ never entitles us to decide that this action of Smith's is a generous action. But we do decide by reference to Smith's character, whether or not this action is characteristic of him. If Smith is said to be generous, and this action is a generous one, then we can explain his action as being characteristic of him. If Smith is generous and on this occasion he acts ungenerously, then he acts uncharacteristically. To say that Smith acts characteristically, or that a certain action is characteristic of Smith, involves saying no more than that this is an instance of what Smith has always, or usually, done. It is what you might expect, knowing what Smith has previously done, that he would do. To say that he acts uncharacteristically is to say that this is an action of the sort that Smith has rarely, or never, done before, and that it is an action of the sort which, knowing what he has previously done, we have good grounds for expecting that he will not do.

What is to be said about the character of the man who acts uncharacteristically ? We must distinguish between the man who only apparently acts uncharacteristically and the man who really acts uncharacteristically. We must distinguish, that is, between the man who has been regarded as honest, and who is now discovered to have stolen the petty cash, and who is also found, on investigation, to have stolen regularly before, and the man who, regarded as honest, steals the petty cash, and on investigation it is found that this is the first time he has stolen anything. In the latter case the man acts uncharacteristically, and we were not wrong in calling him honest. In the former case, the man does not act uncharacteristically. We have been wrong about his character; he was not in fact honest. It might be said, of course, by anyone who insists on the impossibility of uncharacteristic action, that even in cases in which a man acts dishonestly for the first time, after having consistently acted honestly, we could not really have known the man's character, but on this view it would be difficult to see what it would be for us ever to know a man's character.

Of the honest man who steals for the first time, we may say that he is honest, but that he lapsed, and we have no good reason to suppose that he will continue to act dishonestly. That is, we might still be prepared to apply the term ' honest ' to him, in spite of his having acted dishonestly on one occasion. What he tells us of the circumstances of his action often helps us to decide. That he was sorely tempted, or had accumulated debts through no fault of his own, may help us to decide that this was a lapse.

Sometimes, we just do not know what to say. One dishonest act does not make a dishonest man, and one generous act does not make a generous man. Will his future actions be such that we can continue to apply to him the terms we have so far used, or will his subsequent acts be such that we shall have to apply different terms ? We have to wait and see. If his subsequent acts are such that we can apply the same terms, this act will be seen as an exception to the way in which he usually behaves. If, subsequently, he acts, for example, dishonestly more often than honestly we shall withdraw the term 'honest', and say that he is dishonest. We shall then regard this act as characteristic of him now, although it was uncharacteristic of him as he used to be. His character, if you like, has changed.

I have argued that the criteria for deciding whether or not a particular man acted generously on this occasion is never his character; that 'Smith acted generously' is never dependent on 'Smith is a generous man', and this seems to me to be the case no matter what the criteria are. If however, we include among the criteria for 'acting generously', some reference to the reason the agent had for doing whatever he did, *i.e.* his motive, there may be difficulty in knowing that the criteria have been satisfied, for we do not always know what the man's reason was, and we do not always believe him if he tells us. In this connection, there is some truth in the assertion that we do explain people's actions as, *e.g.* generous, by reference to their characters, although none, I think, in the assertion that actions are to be accounted *e.g.* generous, by reference to their characters. There are occasions when, as a matter of fact, having decided that Smith is ungenerous, we use this to explain his subsequent action as also ungenerous. If Smith, who is ungenerous, gives money away because he wants to help, we may refuse to believe him even though he tells us that he did this to help. We may in fact say, 'But he is a mean man; he probably couldn't get out of it'; 'He probably thinks there is something in it for himself'. We should probably, on such occasions be reluctant to believe that he has acted generously, and we should not necessarily be unjustified in refusing to believe this, if he has always acted ungenerously. But this is not to say that we should be right in explaining this action by what he has usually or always done. Because we do not believe that the criteria have been satisfied, it does not follow that they have not been satisfied. This does not mean that the criteria are such that whether or not they have been satisfied is in principle unknowable. Smith knows whether or not he acted generously.

We are, of course, equally reluctant to believe of someone of whom we say, with justification, that he is honest, that he has acted on this occasion dishonestly, but we are often forced to realise that he has. It is often easier to discover first dishonest actions than first generous or ungenerous actions, or first honest actions. Who believes that the criminal refrained from robbing the poor box because he decided to go straight and not because he saw a policeman somewhere about ? Nevertheless, we do have experience of first dishonest actions, and these are uncharacteristic actions. First generous or ungenerous actions are also uncharacteristic actions, and although detection of these is more difficult, we need an account of the relations between character and actions which will allow for these and for first dishonest actions.

There is, of course, a great deal more to be said about such actions, but until we abandon the view that if a man's actions are not predictable from his character they are not 'his', this cannot be said.

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VI.—DISCUSSIONS

THE PREDICTION PARADOX

THIS article is divided into two parts ; the first deals with the so-called " prediction paradox ", the latest attack on which is by Mr. R. Shaw (MIND, 1958, p. 382) ; the second contains an application of my attempted solution of this paradox to the problem of free will. If my dismissal of earlier ' solutions ' of the paradox should appear summary, this is not because I do not admire the skill and esotericism with which the problem has been approached, but because I feel that my approach has the advantage of being so simple that anyone who reads it will agree with the solution.

I

The paradox can be considered in the following form (see Shaw, 1958, Weiss, 1952, etc.) : A headmaster announces to his pupils that it is an unbreakable school rule that an examination will be given on an unexpected day of the next term. A clever pupil reasons that it cannot be given on the last day, because then they would know on the eve of the last day that it could only take place on the morrow, in which case it would not be unexpected. On the eve of the penultimate day, they would know that, since the master cannot wait until the last day, he must give it on the penultimate day, so again it would not be unexpected, and thus it cannot be given on the penultimate day. And similarly, for any finite number of days ; so it would appear that the rules are self-contradictory, and the examination cannot be given. The master sets it, however, say a week after the beginning of term, and this, or indeed any other time, seems to satisfy the rules of the school.

Mr. Shaw, after stating that, to his mind, Quine evades the paradox rather than resolving it, quite explicitly evades it himself. He actually writes : " What rules do we *choose* for the school ? " (p. 383, my italics), and then produces a set of rules which are non-self-referring, and gets no paradox. This is hardly surprising ; but there is no need for him to consider (r + 1) rules, of some complexity, for an n-day term ; he could simply *choose* the rule " There will be an examination on one of the days of next term " which is both non-self-referential, and leaves the situation such that the pupils will in fact not know on which day it will take place. Another excellent way to avoid paradoxes is to say nothing ; if the master does this he may offend the boys but he can be sure that he will not offend logic. People with no rules at all are always safe in so far as they cannot contravene them. Compare " A contradiction cannot occur in nature ". But contradictions neither occur nor fail to occur *in nature*, only in linguistic expressions. What this means is that if, in argument, we reach a contradiction, then either we have argued incorrectly, or one of the premisses either contradicts another premise, in which case one of them is false, or is itself self-contradictory. I show below that the school rule is ambiguous; if it means one thing then the clever boy argued incorrectly, if another, then it has no empirical application because it is self-contradictory, and the apparent instantiation of the rules is not an instantiation at all. What is so puzzling about the paradox is that the rule appears to be both self-contradictory and yet instantiated at the same time; naturally, we can get out of this difficulty by choosing a *different* rule, but this is absolutely nothing to the point. We wish to find out what went wrong with the rules *as stated*; surely they cannot be self-contradictory, since they were instantiated! So it would seem that there must be something wrong with the clever boy's argument. But what? Here, as so often in philosophy, the search for the general is the illusion; we look for *the* answer, instead of different answers depending on the way the rules are taken.

It is unclear to me whether Mr. Shaw thinks that we are bound to get paradoxes if we use self-referential statements, perhaps because we are confusing object-languages and meta-languages, but he says "It is clear that the origin of the paradox lies in the self-referring nature of Rule 2*" (p. 384). But many self-referring sentences are perfectly all right, *e.g.* "This sentence is written in black type", which will be true or false, or "Many people expected a paper on expectation paradoxes to appear in MIND, but no one expected *this* paper". The last sentence refers to this paper and thus to itself, as does this sentence (twice). Moreover it is true. I wish, however, to take Shaw's Rules 1 and 2* and show that no paradox need arise. Although the apparent paradox arises over an ambiguity in the phrase "you will be unable to predict", or "you will not know", I cannot accept Quine's solution, for by his criterion I could never be said to know anything about the future. This only means that whenever I say "I know that X will happen" it is logically possible that X should fail to occur, despite all the predictions of hanging judges, schoolmasters, scientists, angels, or prediction machines. For how could I know about the future better than I do when a judge tells me that he will hang me on the morrow, or when a master tells me that he will give me an examination on the morrow in accordance with an unbreakable school rule? True, the rule, like an insurance policy, does not allow for civil riots or acts of God, but it is not in this sense that it is said to be unbreakable.

Weiss is almost correct when he says that if the announcement is made on the eve of the penultimate day, then "it is not predictable which of two days will be the day on which the exam will be given",[1] but this is not because "There is as yet no distinct next day or day after on which the exam could be given", nor because, of $f(x \vee \bar{x})$ and $f(x) \vee f(\bar{x})$, "the former is a necessary truth, the latter is true only when one has isolated the x and the non-x, an act which requires one to leave the realm of possibility for the realm of time, history, becoming" . . .

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[1] MIND (1952), p. 267.

Now suppose that a man A, holding six black cards and one red card in his hand,[1] says to a man B "I am now going to lay the cards one at a time face upwards on the table in front of me. You will not be able to predict, before I turn it over, which time I am going to play the red card." B replies, "Well, you cannot leave the red until last, because then I would know, and you cannot leave it until last but one because then, knowing that you could not leave it until last, I would know that you were going to play it last but one, and then you cannot etc. etc. etc. . . . and therefore you cannot lay it at all! In fact, come to think of it, if you hold 52 cards, all different, and say that I cannot predict which card you will lay, then you have contradicted yourself. For, if you leave the ace of clubs until last, I would know that you were going to lay it last, and so you cannot lay it last. Therefore neither can you lay it one but last, nor 52-n but last, ($n = 50, \dots 1$), and so you cannot lay it at all. And similarly for all the other cards."

The absurdity of this argument is manifest. When A holds just one black and one red card, and says to B "You cannot predict which card I shall lay", he means that B cannot predict the first card, not that B will be unable to predict the colour of the second card when he has seen the first. Or if he does mean this, he is just wrong, as B will quickly show him. Similarly, when, on the eve of the penultimate day, the master claims that the boys cannot predict which day the examination will be given, he does not mean that *if* he does not give it on the morrow the boys will *then* be unable to predict that it will be on the last day. Or *if he does mean this, then what he has said is false, and his giving it on the first day does nothing to prove him right*, for it simply fails to satisfy his claim that "the date of the examination is unpredictable even if we wait until the eve of the last day".

The headmaster may perfectly well claim that the rules just *are* Shaw's self-referring and unbreakable[2] R1 and R2*, *viz.*:

R1 An examination will take place on one day of next term.

R2* The examination will be unexpected in the sense that it will take place on such a day that on the previous evening it will not be possible for the pupils to deduce from Rules 1 and 2* that the examination will take place on the morrow.

What the headmaster surely cannot deny, without contradicting himself, is that R2* entails, or if you like, means, either S1 or S2, *but not both*, where these are:

S1 The examination will be unexpected in the sense that . . . it will not be possible for the pupils to deduce from Rules R1 and S1 that the examination will take place on the morrow, unless it takes place on the last day.

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[1] I believe that I owe this example to Mr. S. Anstis.

[2] This cannot mean logically unbreakable; they must allow for epidemics, wars, or sudden chaotic cancellations of natural laws.

S2 The examination will be unexpected in the sense that . . . it will not be possible for the pupils to deduce from Rules R1 and S2 that the examination will take place on the morrow, even if it takes place on the last day.

If it means S1, which is the more sensible, then the argument put forward by the clever boy is fallacious, because the rule, applied for instance on the eve of the penultimate day, states merely that the boys will not know on which day the examination will take place *unless* the master waits until the last day. If it means S2, then it can have no possible application, must always remain false, for nothing, including setting the examination earlier, would make it true that the boys would be unable to deduce on the eve of the last day that it would occur on the morrow, *if* the master were to wait that long. For R1 and S2 applied together on the eve of the last day give us :

(1) The examination must take place tomorrow.

(2) (The examination will be unexpected in the sense that) it is not possible to deduce from (1) and (2) that it will take place on the morrow.

(1) and (2) clearly contradict each other, as opposed to Quine's solution. The latter's use of 'know' ('predict', 'expect', etc.) such that I could never know anything about the future, is just as implausible as that opposite use whereby a man who guessed the day correctly and then said that he had predicted rightly might be said to have known. (See further discussion of the card game in Part 11.) Shaw is correct in saying that we are only interested in *valid inference*, in this case deductive as opposed to inductive, etc.

Thus if the rule means S2, there can be no instantiation of this rule, only an apparent instantiation, and the boy argued correctly, if he argued that *this* rule could not be instantiated. Of course the master can *say* that the rule means S2, or even S1 and S2 at the same time, and still set the examination, just as I can make the self-referring, self-contradictory remark "I always lie, including now", and then spend the rest of my life telling lies. But in neither case are we instantiating our statements, for nothing could instantiate a self-contradictory statement.

The paradox arises from taking the rule to mean both S1 and S2 at the same time. The master might claim that the rules in the rule-book do not state which it is meant to mean ; but what is certain is that it cannot mean both without being self-contradictory from the start. In this case, like all self-contradictory statements, it can do no harm, it can hurt no one, unless it misleads them into wrongly thinking for instance that there will be no examination, when what they ought to do is expect one, and thus revise, on *every* night.

II

I would like now to consider an application of the prediction paradox to the problem of free will. Certain philosophers have claimed that we must have free will, or that we certainly often know that there is a goldfinch in the garden or that someone is in pain, etc. etc., because in using the relevant words to describe these situations we are only claiming that the situations easily referred to when we utter these words, do occur. No one likes to drag a corpse from its coffin and then kick it, so I will say no more about this except to refer to the various articles on the Paradigm Case Argument in ANALYSIS during 1958. The question now arises whether the situations paradigmatically described as ones of knowledge, free will, etc., really are correctly so described, when certain beliefs, such as that all knowledge comes through private data, or that there are causal universal laws, seem to conflict with these descriptions.

I do not wish to discuss here whether and in what sense I could have chosen to have chosen . . . to choose differently in a given situation, and whether this lands us in an infinite regress. Neither do I wish to consider whether certain machine-states are essentially unpredictable because of Gödel's theorem. All I want to do is to take a very simple situation, discuss in what sense it is predictable and whether this conflicts with the belief that we have free will, and then to generalize this case.

I presume that no one would deny that in the case of the card players considered above, there is a sense in which A, when he holds but two cards, can play whichever one he chooses. If I understand them correctly, the ordinary language philosophers, by contrasting this sort of case with those in which A is compelled because someone holds a pistol to his head or forcibly moves his hand, declare that free will exists since these are the sorts of case everyone refers to when they use the words "free will". The question whether, as some "metaphysicians" have maintained, this use is a misuse because incompatible with there being universal causal laws, is straightaway ruled out. I shall not repeat the overwhelming arguments against the ordinary language view, but will immediately attempt a consideration of the metaphysician's claim.

Firstly, the mere idea of determinism *per se* does not seem to conflict with our notion that we have free will. Card-player A, if he can decide freely whether to play the red card or the black, and then play it, does nothing contrary to the laws of nature such as float up out of his chair and hit the ceiling ; if he did, he could not play the card which he chose, which would hamper his freedom of choice very badly. "Everything which happens does so in accordance with the laws of nature" tends towards analyticity, not because it is one of those statements which Quine and Waismann claim are semi-analytic—for none are—but because we tend to *make* it express a necessary proposition by not allowing anything to count against it. There are excellent pragmatic reasons for doing this, but if we give it up because we have reason to believe that no laws could be found in certain cases, it does not mean that the proposition which the sentence used to express is now false.

Let us therefore assume that it is true ; we have then granted the metaphysician as much as he could possibly want. Now the feeling that this destroys free will might be expressed by A as follows : " Whether I play the red card or the black, my action was determined by causal laws, and so one who knew enough about me and about the laws concerned could have predicted which one I was going to play first, and there is nothing which I could do to frustrate him." We might immediately reply : " Yes there is ; you could lay the other one, for we have surely admitted that to lay either card would be in accordance with the laws." A might reply : " But in that case, the Ace Predictor would not have predicted correctly, which is contrary to hypothesis. He knows the truth of all statements P_1 about world-state W_1 at time t_1, and he knows the laws of nature L ; these, taken together, entail that the world-state W_2 at t_2 shall be P_2, and one of the statements P_2 will be ' A lays red ' or ' A lays black ', which the Ace Predictor could have foreseen. So all the future is in this way laid out before me, and I must follow the paths which fate has laid down for me."

Now does this only make the tautological claim that ' whatever will be, will be '[1] or does it claim more ? Suppose that B, when he was arguing against A, had said that he could predict which card A was going to lay first. A might say, ridiculously enough, " Well, which card *am* I going to lay first ? ", and when B says " Black " A always lays red, and *vice versa.* I believe that this is all that A could possibly hope for when he claims that he has free will, *i.e.* that *whatever* the Angel of Fate or the Ace Predictor predicts, then if A gets to know what has been predicted, what is written in the Book of Fate, he can act otherwise, thus destroying the power of the Book.

The Angel of Fate might then say, " Of course that wasn't the *real* Book of Fate ; the real one foresaw what you would do when I showed you this one, and in fact predicts everything about your life correctly ". But this is surely only worrying to A if he can be shown the real book of predictions, and he then finds that he cannot help acting in accordance with it because he is somehow constrained or impelled. If the angel replies that the book is kept in heaven and that only God is allowed to look at it, this is no more impressive than if B says, after A has laid the red card, that although he said " Black " he really thought all along that A would lay red.

To make the game fair to both A and B, B should write his prediction on a piece of paper ; this would then be compared with the card after A has laid it. B may, by guessing, always be right ; we should then be mystified, but why should we be any more disturbed if B says that he asks the Angel of Fate, or that he pushes lots of data about neurons, etc., into an enormous computing machine ? It is still the case that A, if shown the prediction, could act differently. If two people B and C are both making predictions about which card A will lay, and if they agree always to differ, then one of them must be right every time. And if A lays n cards, or performs n actions with m possible alternatives each time, then of m^n possible predictors all predicting differently, one of them logically must be right. That one will be right is no more disturbing than that B should write his first prediction on two pieces of paper, and then after the event produce the one which corresponds with the card that A has lain. In this 'guessing' sense of "predict", it is always false that a person is unable to predict what another will do. Thus when the master says that the date of the examination is unpredictable he does not mean that the boys will be unable to say each evening "Exam tomorrow" and one day be right; indeed he wishes them to say it every night, so that they revise every night.

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[1] See G. Ryle, *Dilemmas*, chap. 11.

This sense of 'know' or 'predict' is one diametrically opposed to that used by Quine; we are more interested in a more rational, intermediate sense, where it means "validly infer on deductive or inductive grounds". But even if an Ace Predictor can do this, it still is the case that if A sees part of the machine-tape referring to the future, he can decide to act in discordance with it, if the specifications are precise enough. For instance, suppose that the tape says "A dies 28 Jan., 1962, London"; A may unfortunately die when the machine states, just by coincidence, but he can cheat the machine by taking a train to Liverpool and refusing to move. We must surely admit that such actions are possible, *i.e.* that if someone makes a prediction about our behaviour and then tells us about it, we are very often able to act otherwise. And if some statement about determinism is even analytically true, then this only makes it analytic that the behaviour immediately after viewing a predicting-tape is explicable in terms of a law of nature which probably would not even have been instantiated if the predicting-tape had not been seen.

Thus the Book of Fate kept in heaven, which only God is allowed to see, is as unworrying as the boy's prediction of the examination or the devil's prediction about the cards, whether these are based on natural laws or inspired guesswork. Suppose that the predicting angel gave me a book and on each evening provided me with a key to turn another page, whereon I always found a minute description of all that I had done that day. The angel declares that the story of my life is printed in the book, and that when the book ends, so will my life. At first I might suspect that he is cheating, as B did, when he wrote on two pieces of paper. For he might be noticing everything that I do and then transmitting it daily onto the as yet blank pages by means of invisible angelic high-frequency waves. But as the pages of the book get fewer, I might get more worried; eventually when very few pages are left, I try to break the lock in order to see ahead and escape my doom. If I cannot break the lock, the performance of the Angel of Fate is only startling in that he can make strong locks and use invisible rays, and possibly that he makes one correct prediction, *viz.* the date of my death. If I do open the lock, and see that the moving finger has got there before me, I can take evasive action and thus cheat the moving finger. It is surely irrelevant whether I or the moving finger always act in accordance with the laws of nature or not.

Thus it would seem that neither determinism nor indeterminism is incompatible with free will, and that the apparent conflict arises through not examining what it is for a thing to be predictable, that is, through not examining particular-case uses of the word " predict " —more and less complex. Once again we tend to rely on a vague, general gesture in the direction of what it is for such and such to be the case, as though we could understand the meaning of the word apart from its particular case-by-case uses, ordinary and extraordinary. If we examine particular concrete examples, real or imaginary, such as that of the card-players, and each time ask ourselves in what sense a move is predictable, and in what sense this is, or might be, incompatible with choosing freely, we will see, I believe, that the worry about the Ace Predictor should be laid to rest alongside that of the Arch Deceiver. For each moment of our lives it is as though we play another card, determined partly by those we have played so far, partly by what we have been dealt, and partly by free choice from those we still hold.

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ON SUBJUNCTIVE CONDITIONALS WITH IMPOSSIBLE ANTECEDENTS

By a subjunctive conditional I mean here a statement of the following kind :

' Whether or not p is true in fact—if it *were* true, then q would certainly (or necessarily) be true ' ; or, perhaps, ' should p hold, then q would hold ', etc.

One can use the symbol

$$`p \underset{s}{\supset} q'$$

as an abbreviation of a statement of this kind. (This may also be read, if one prefers, ' p subjunctively implies q ', in analogy to reading ' $p \supset q$ ' as ' p materially implies q '.)

The question to which this note is devoted is this : Let us use the name ' opposite subjunctive conditionals ' two subjunctive conditionals with the same antecedents and directly opposite consequents, such as

$$\text{(i)} \qquad p \underset{s}{\supset} q$$

$$\text{(ii)} \qquad p \underset{s}{\supset} \sim q$$

We may then ask : can two opposite subjunctive conditionals be true together ?

There seems to be considerable disagreement on this question. In an appendix to my book *The Logic of Scientific Discovery* (new appendix *x), I implicitly assumed, without argument, that all those interested in subjunctive conditionals would agree that (i) and (ii) could well be true together, provided p was (physically) *impossible.* (I even assumed more than this.) I soon had to learn that some who had thought much on the subject disagreed with me on this point.[1]

Without claiming to say anything new here I shall therefore attempt to establish, by purely intuitive argument, two things.

(1) We can construct consistent sets of premises from which two opposite subjunctive conditionals, related as (i) and (ii), may be deduced by valid intuitive arguments.

(2) As a consequence of (1), we are bound to admit that two opposite subjunctive conditionals can be true together. If this admission is felt, by some, to be counter-intuitive, then we may ask them to remember that there are certain unusual and rarely used

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[1] See the challenging paper by P. B. Downing, ' Subjunctive Conditionals, Time Order and Causation ', *Proc. Arist. Soc.* (1959), pp. 125-140. (Meeting of the Society of 2 February 1959.) Note especially the remark on p. 126 : ' It seems clear, though it is perhaps impossible to prove, that two subjunctive conditionals with the same antecedent and contradictory consequents cannot both be true.' If I am right, then this assumption, though intuitively persuasive, can be disproved.

cases ('zero-cases' or 'points of singularity') upon which it is unwise to bring intuition to bear immediately.

Before proceeding to an empirical example (II), I shall sketch an arithmetical example (I).

Consider the following three compatible premises, expressed in what may be called the 'subjunctive style'.

(I) Should a certain number a be a natural number between 3 and 6 (*i.e.* $3 < a < 6$), then the following would hold: should $a \neq 4$, then $a = 5$, so that a would be a prime number; and should $a \neq 5$, then $a = 4$, so that a would not be a prime number.

(IA) Should $a > 10$, then $a \neq 4$

(IB) Should $a > 10$, then $a \neq 5$

Moreover, we state categorically as a fact (fourth premise):

(I+) $a > 10$

Now two people, Allen and Brown, are asked to consider, in the light of these premises, the following question: if (*per impossibile*) a were between 3 and 6, would it be prime or not?

Allen may then argue from (I), (IA), and (I+), that a would be prime; and Brown from (I), (IB), and (I+) may argue that a would not be prime.

I believe that both deductions, however trite, are correct. I further hold that the situation is precisely analogous to the case where we can deduce two opposite material conditionals $p \supset q$ and $p \supset \sim q$ from consistent premises. There is no problem here: we simply conclude in this case that, in the light of the premises, p must be *false*. Similarly with subjunctive conditionals: we simply conclude that, in the light of the premises, p must be *impossible*.

To this argument, the reply has been made that reasoning from arithmetical examples is unconvincing in questions of this kind. I therefore proceed now to an empirical example. We consider the following three subjunctive premises:

(II) If Peter were now in his flat, he would be either in his bedroom or in his bathroom, and he could not be in both at the same time, and he could be in no other room (say, because the flat has no other rooms).

(IIA) If Peter were now about twenty miles from his flat, he could not possibly be in his bedroom now.

(IIB) If Peter were now about twenty miles from his flat, he could not possibly be in his bathroom now.

Moreover, we have the factual premise (fourth premise):

(II+) Peter is now about twenty miles from his flat.

We ask Allen and Brown to consider, in the light of these premises, whether or not Peter would be in his bathroom now if (*per impossibile*) he were now in his flat. Allen argues from (II), (IIA), and (II+) that, if Peter were now in his flat, he would now be in his bathroom; while Brown argues from (II), (IIB), and (II+), that if Peter were now in his flat, he could not possibly be now in his bathroom.

Again, I think that both arguments are valid.

Now it is clear that our two sets of premises are both consistent, since they are both satisfiable : if we put $a = 11$, our first set of premises becomes true. Similar, the second set of premises was, in fact, true of my friend Peter M. some short time ago. Since from true premises only true conclusions can be validly derived, we are bound to admit that two opposite subjunctive conditionals, related like (i) and (ii), can be true together.

To my own mind, this fact is far from counter-intuitive. But since there are people who find it counter-intuitive that two opposite material conditionals can be true together (or perhaps that 'Every Pegasus has wings' and 'No Pegasus has wings' might be true together), it is to be expected that there will be people who find our result intuitively unacceptable.

Yet I feel that our intuitions are not always to be trusted in cases which simply do not arise, or which are avoided, in ordinary life, or in ordinary parlance. Perhaps the simplest case of failure arises in connection with the question : is zero a number ? There is no doubt that, in ordinary parlance, it is not. 'I have a number of pennies in my pocket, and the number is zero' would be eccentric in ordinary parlance. (In fact the smallest number that makes 'a number of', in ordinary parlance, is three.) This example is not as trite as it looks : the extension of the idea of number from *many* to *two*, and to *one*, and ultimately to *zero*, is a feat of abstraction which was not easy to perform : we still count 'one, two, three', rather than 'zero, one, two, three'. Yet while the question whether we should call zero a number is a comparatively unimportant terminological question, grave and startling conclusions may be drawn from the assumption (as may be drawn from any logically false assumption) that two opposite subjunctive conditionals, since they cannot be true together, cannot both follow from consistent premises. One such startling conclusion would be that Brown has succeeded in refuting Allen.

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A NOTE ON MR. MACINTYRE'S *DETERMINISM*

Mr. MacIntyre (Mind, January 1957), pointing out the growing success of science in explaining human action, argues that this is relevant to the problem of Free Will, and cannot be discounted simply by arguments (*e.g.* those of Professor Flew) derived from paradigm usages of terms like 'free'. Insisting in this way that there is an incompatibility between the growing evidence for Determinism in human action and the notion of Free Will, he then proceeds to give an *a priori* argument against Determinism as applied to human action.

While I accept the incompatibility which Mr. MacIntyre argues for, I wish now to argue that his *a priori* way with Determinism will not do. He suggests (p. 34); "that we can meet this case [the case of the sciences' having continued success in the explanation of behaviour] by means of a concept which will allow us to admit both the indefinite possibilities of psychological and physiological discovery and the fact that such discoveries may be legitimately added to our list of negative criteria without, however, conceding that we may see the area of human freedom dwindle indefinitely. This is the concept of 'rational behaviour'. Behaviour is rational—in this arbitrarily defined sense—if, and only if, it can be influenced, or inhibited by the adducing of some logically relevant consideration." Certain areas of action are, by this argument, brought under what even Mr. MacIntyre admits to be an arbitrarily defined notion; from the terms of the definition it is then inferred that this class of actions must be uncaused. How is this inference defended? It is held (p. 35) that it follows from the definition of rational behaviour "that if a man's behaviour is rational it cannot be determined by the state of his glands *or any other antecedent causal factor* [my italics]. For if giving a man more or better information or suggesting a new argument to him is a both necessary and sufficient condition for . . . changing his mind, then we exclude, for this occasion at least, the possibility of other sufficient conditions. . . . Thus to show that behaviour is rational is enough to show that it is not causally determined in the sense of being the effect of a set of sufficient conditions operating independently of the agent's deliberation or possibility of deliberation."

Now it is clearly the case that if it is true that someone acts solely as a consequence of deliberating logically relevant considerations, then it is not true that he acts either solely or partly as a consequence of the state of his glands, etc., etc. But this has not the least tendency to show that if someone acts rationally then his behaviour "cannot be determined by the state of his glands *or* any other antecedent causal factor". For obviously the Determinist will not deny that people often act as a result of deliberation, *i.e.* rationally in Mr. MacIntyre's sense; but will hold that "deliberation" marks out a process in which each earlier state causally determines each later state, and which in turn causally determines those actions which are performed as a result of deliberation.

It should be noted that even this answer would not be necessary in many cases which Mr. MacIntyre would class as rational. For "rational behaviour is defined with reference to the possibility of altering it [by logically relevant considerations]" (p. 35). Thus, as Mr. MacIntyre fully admits, there are many instances in which the agent does not deliberate at all, but acts on the spur of the moment ; yet nonetheless still acts rationally. Now in such cases as these Mr. MacIntyre cannot hold that the action proceeds from no antecedent causal factor, for since *ex hypothesi* it does not proceed from deliberation either it would then be random, and he admits[1] the cogency of the arguments aimed at showing that random behaviour, if it occurred, would not be free, nor hence, in his sense, rational behaviour. He must therefore allow that sub-class of rational behaviour in which no deliberation *in fact* occurs to have causal antecedents, in which case it drops out of the argument against Determinism ; or else he must admit that it is random, in which case the premiss that it is rational, in his sense, is defeated. So it is only necessary for the Determinist to concentrate his attention on those cases where deliberation *does in fact* occur.

So let us return to Mr. MacIntyre's main case in which rational behaviour *does* proceed from deliberation. He takes full cognizance of the Determinist reply I suggest above ; " . . . in the widest sense of the word 'cause' the giving of a reason may function as a cause" (p. 37). But he rejects the argument on the following grounds ; there is a difference, he argues, between "a giving of reasons which is causally and a giving of reasons which is rationally effective" (p. 38). My reasons might persuade you because they were propounded in a convinced tone, but this would not be a case of rational behaviour. From this fact Mr. MacIntyre infers that "to act because you were given reasons to act would not necessarily be to act in a causally determined way. It would only be necessarily the case that to act as a result of being given reasons to act was to act as a result of determining causes, if we counted as a cause of behaviour anything which influenced behaviour at all. If we understood the expression 'cause of behaviour' in this way, to say that our giving of reasons supplied a sufficient determining cause of behaviour would be to utter an empty tautology" (p. 38).

Now while it is true that there is a distinction between reasons which are *merely* causally effective and reasons which are rationally effective, this fact goes no way to meet the Determinist argument that rationally effective reasons are also causally effective. Let us hold with Mr. MacIntyre that for the Determinist to classify as causal factors the giving and deliberation of logically relevant reasons, *if* he did not also think it possible to describe completely such giving and deliberation as *causally* effective, would involve him in making his case by a definition of *anything* influencing behaviour as a cause of behaviour. But even on this assumption, the argument must fail, for it is precisely what the Determinist *is* suggesting that what happens when someone gives reasons, and when someone else deliberates them and acts on them, is that a sequence of causes and effects occurs, in which the last term is the action in question. (This *seems* to require a discussion of the mind-body problem, and the establishing of some sort of Epiphenomenalist or Behaviourist view, in which physical causes are the only causes of behaviour. But I do not think this is so. For even if Interactionism were true, which I think highly unlikely, the Determinist could still maintain that those mental occurrences which are postulated as occasional determinants of behaviour *were* determinants of behaviour, *ex hypothesi*, in the same sense as physical determinants of behaviour.) And this seems to me to be a very likely suggestion. For I see no reason to believe that the series of events marked out by such a description as "He acted in such and such a way because he had taken to heart the cogent reasons advanced by his friend" cannot be redescribed, without loss, in terms of causally determined sequences of events. It may be urged that two utterly disparate ways of speaking are in question. No doubt; but why should precisely the same situation not be described in two utterly disparate ways?

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[1] " . . . to say that any given event is uncaused is surely to say that such an event is random. What is random is no more free than what is caused" (p. 30).

Mr. MacIntyre returns to say why with this argument; "if a man confronts a moral problem, weighing up the pros and cons of a situation, to depict his judgement on it as the effect of a causal impact made by the situation would do violence to all our ordinary ways of talking about morality" (p. 39). That is, it is normal to *contrast* unthinking action, or action proceeding from emotion alone,[1] with action proceeding from careful and calm consideration of the factors of a situation. Mr. MacIntyre adduces as analogous the progress possible under psychotherapy from stubborn compulsive reaction to awareness of compulsiveness and its undesirability by comparison with other more rational techniques for decision-making. Mr. MacIntyre's argument, then, so well as I can understand it, is that the Determinist position is involved in assimilating what we ordinarily seek to distinguish. It is hard for me to see what force, suasive or demonstrative, it possesses. For if any piece of deliberative behaviour, of no matter what complexity, can also be represented as a strict causal sequence, then that's that, and so much the worse for "all our ordinary ways of talking about morality". And that it can be so represented seems not unlikely. There is a great difference between the machine which shoots out a chocolate when a sixpence is put in it, and the machine which calculates, on the basis of range, direction and velocity of target, velocity of gun, wind-speed and direction, barometric pressure, etc., etc., where a shell must fall, fired from a moving gun, to strike a moving target. But this vast difference in complexity has no bearing whatsoever on the question whether in each case the end-product is or is not strictly determined by a series of steps, themselves all strictly determined. I intend the analogy in a double sense ; first as showing that the difference between a largely automatic response to a stimulus and a response involving manipulation, according to prescribed rules, of a variety of highly complex data does not touch on the question of Determinism or Indeterminism; second to draw attention, in passing, to a well established scientific hypothesis concerning the causation of deliberative behaviour—I mean the hypothesis of Cybernetics. The very difference between the sixpence-in-the-slot machine, and the gun-laying computer, *viz.* that between a purely Newtonian machine, and one embodying the feed-back principle, may well prove to be roughly the difference between a reflex or habitual or impulsive action and a carefully deliberated action.

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[1] That Mr. MacIntyre should make such a distinction, implying that there is no objection to assigning causes to the first sort of action, lends further importance to my earlier argument aimed at showing that unreflective action cannot be rational in his sense.

That is one sort of reply to Mr. MacIntyre's argument ; but a different sort is also possible. For he suggests that the distinction (between unreflective and reflective behaviour) on which his present argument rests would somehow be broken down, or shown to be unreal, by a successful causal account of the latter type of behaviour (" would violate . . . "). He seems to strengthen his case by speaking of a successful Determinist account of deliberative behaviour as depicting the " judgement [of a man who has weighed pros and cons] as the effect *of a causal impact made by the situation* [my italics] " (p. 39). This last is in fact a complete misdescription of the sort of causal account of deliberative action that I have suggested—a Determinist *au fait* with modern science would not maintain that the " causal impact of the situation " is of some simple S-R sort *both* in the case of reflex or drilled *and* in that of deliberative action. But by improperly suggesting that this is so, Mr. MacIntyre seems to strengthen his argument that somehow a causal account of deliberative action *would* break down the distinction between it and reflex or drilled or impulsive action. There *is* a difference between responding unthinkingly on the one hand, and carefully weighing all the relevant data on the other. This difference could be finally established merely by a full description of the two sorts of activity ; but that we should be able to make it is a fact which is untouched by any causal account we find can be given, even if it were a purely S-R account, as Mr. MacIntyre incorrectly suggests it is or must be. *Even if* the S-R account were the correct one—that is, when the argument is at its most persuasive—I do not see that this would in any way break down the ordinary distinction that is in question. In fact, however, if the feed-back hypothesis, or something like it, is correct, then the distinction is reflected in the quite different explanatory principles required for the two sorts of action.

As against Mr. MacIntyre, then, I have argued that even if a causal account of both types of behaviour *did* break down the ordinary distinction between them, that would not be an argument against the correctness of the causal accounts. I then argued that there was anyway no reason to think that *any* causal account *would* break down that distinction.

A further argument is proposed against the Determinist programme of assigning causes to rational action, but I find it equally unconvincing. It is the alleged difficulty in seeing " how determinism could ever be verified or falsified " (pp. 39-40). Suppose, runs the argument, a complete causal explanation of my behaviour given. Suppose too my behaviour to be fully rational, in the defined sense. " Then no test will be available to decide whether I act as I do because it is the rational way to act or because it is the way in which my deeds are causally determined. . . . Whereas the contention that my behaviour is determined by causal factors is normally taken to mean ' determined by causal factors as contrasted with rational appreciation, etc.', here ' causal factors ' have nothing to be contrasted with and hence the expression ' determined by causal factors ' has been evacuated of its customary meaning " (p. 40).

But the argument does not support its conclusion. The antithesis Mr. MacIntyre points to between behaviour proceeding from casual factors and behaviour proceeding from rational appreciation is presumably co-extensive with that between behaviour proceeding from non-rational and behaviour proceeding from rational sources. (This seems to follow from his willingness to allow that all non-rational (in his sense) behaviour can very probably be assigned causes.) So that whatever the " customary meaning " of " causal factors " is, the meaning is *not* the same when (1) Mr. MacIntyre contrasts caused and rational behaviour, and (2) when the Determinist asserts that *all* behaviour, including rational, is caused (susceptible to a causal explanation). For the Determinist does *not* mean " non-rational ", but " caused " in the prescribed general metaphysical sense, which stands in contrast to the sense of that cluster of Indeterminist positions which hold in one way or another that some stretches of behaviour are *uncaused*. The Determinist is therefore not committed to the absurdity of holding that all behaviour is caused in a sense which would involve that even paradigmly *un*caused behaviour was caused. This means in turn that the general conclusion that Determinism would be neither verifiable nor falsifiable is unsupported, since the required supporting argument that " caused " in the Determinist sense has been emptied of meaning cannot be made good.

Mr. MacIntyre describes, to reject, another Determinist argument aimed at showing that rational behaviour might also be caused behaviour. It is the argument, suggested by psycho-analysis, that all behaviour is irrational, only *appearing* rational in the required sense, and hence probably susceptible to causal explanation. I do not wish to defend this argument.

In this paper I have been arguing that Mr. MacIntyre's *a priori* way with Determinism will not do. I do not want, in this note, to describe in detail what I think is a correct approach to the problem. I should like, however, to make the following suggestions.

(1) Arguments, like those of Professor Flew, quoted by Mr. MacIntyre, show that Determinism is compatible with *ordinary* and *juridical* notions of freedom, in so far as these must provide ways of deciding the *practical* question of where reward and punishment must fall.

(2) It cannot be the case that advances in science, showing more and more areas of action where detailed causes are assignable, should indefinitely narrow the field of free action, in the ordinary sense. This because it is beyond doubt that many actions, caused or not, can be influenced according to our requirements by reward and punishment.

(3) There is, however, a metaphysical notion of freedom which consists, roughly, in the beliefs that (*a*) certain actions cannot be explained entirely in terms of preceding states of the agent, and (*b*) that such actions are fully responsible and the paradigm cases of moral choice. This metaphysical notion of freedom is incompatible with the hypothesis of Determinism. Hence continuing success in explanation of behaviour means an indefinitely narrowing field of free actions in this sense. It is this sense of freedom that makes philosophers' attempts to show Freedom and Determinism to be compatible seem always unsatisfactory.

(4) Arguments of a Humean sort designed to show that actions, free in this sense, could not also be responsible, may (or may not) work. They do not, however, show that this is not the generating model of the Free Will problem.

If a conflict rises from a powerfully fixed pre-scientific view of human nature, it can only be resolved by looking that view full in the face ; then discarding it.

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MR. HARSANYI ON HYPOTHETICAL IMPERATIVES

In Mr. Harsanyi's article, 'Ethics in Terms of Hypothetical Imperatives',[1] the crux of his argument is as follows.

We must distinguish between causal and formal (*i.e.* non-causal) hypothetical imperatives (p. 305). In the causal, the apodosis is a means to achieving the end or preference stated in the protasis. But in the formal, the apodosis is a suggested behaviour which at the same time satisfies the formal criteria expressed in the protasis. In trying to show that moral rules, being in effect hypothetical imperatives, would best be founded on the 'true' preferences of the addressee, Harsanyi, if I have understood his argument, wants also to show that all moral rules do make this reference, and in so doing are analytic. This, he feels, is the surest way to reach an ethics which is ultimate, consistent, and, as far as possible, efficacious or persuasive. Since everyone wants to be treated in accordance with his own preferences, then if all moral rules are based on a person's 'true' preferences, the result is harmonious moral behaviour (presumably).

How well does Harsanyi establish the analyticity of a 'true' preference moral rule? Is it the case that a moral rule which in the protasis refers to a 'true' preference is really analytic? If so, then the moral rule is a formal, or non-causal, hypothetical imperative. The whole argument rests on this point. But Harsanyi's distinction of hypothetical imperatives is oversimplified. The distinction, as far as it goes, is valid, but it is incomplete. Consider the following three hypothetical imperatives:

A. If you want to graduate, then study for your exams.
B. If you want to climb the highest mountain in Europe, then climb the Mont Blanc.
C. If you want to be polite, then call him, 'Sir'.

Now in A the apodosis is clearly a *means* of attaining the goal expressed in the protasis. Studying is not graduating, but is only a means thereto. On the other hand, in B, climbing the Mont Blanc is *equivalent* to climbing the highest mountain in Europe. The two expressions have the same denotation, which is not the case in A. But in C, the situation is much different. Harsanyi includes C in the same class as B, but this is a mistake. For observe that if we formulate A and B as follows:

A. Graduating $\supset$ studying.
B. Climbing the highest mountain $\equiv$ climbing the Mont Blanc.

Thus if one does not study, then presumably he does not graduate. But we cannot deny the apodosis of C and thereby have a denial of the protasis, as in A. Am I impolite because I do not call someone

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[1] Mind, July 1958, vol. lxvii, no. 267, pp. 305-316. All page references will be included in the text of this discussion.

'Sir'? Not necessarily. Now in B, the implication is reciprocal: if someone wants to climb the Mont Blanc but does not know which mountain it is, we can tell him to climb the highest mountain in Europe. And to deny either expression is to deny the other. But this cannot be done in C. For if we formulate C:

C_1. Being polite $\supset$ calling him 'Sir'; (or)
C_2. Being impolite $\supset$ not calling him 'Sir',

neither of these are necessarily true. I do not call my brother 'Sir', nor am I being impolite by not doing so. So we find that C is not a causal hypothetical imperative as A is; on the other hand, it is not a formal hypothetical imperative as B is. How then can it be considered formal in any sense? Only in so far as calling someone 'Sir' is *one* form of being polite, and even then not in every case. One may even be impolite by calling someone 'Sir', depending on the circumstances. Thus the equivalence of C is *arguable* where that of B is not. No one disputes that Mont Blanc is the highest mountain in Europe. This (B) is what I will call an 'analytically' formal hypothetical imperative, whereas C is not. For in order that C be formal at all, much more needs to be considered: the persons involved, the occasion, the class attitude, etc. But these make it 'synthetically' formal, so to speak. Hence the bare distinction of all imperatives into causal and formal is inadequate and insufficient.

What effect does this oversight have on Harsanyi's thesis? He tells us: 'Moral rules do not tell us what to do in order to produce certain particular results [as in A above], but rather tell us what to do to make our behaviour . . . satisfy certain formal criteria' (p. 309). But is such a rule in form B or in form C? Harsanyi surely wants it in form B. Thus in an example he suggests, 'If you want to follow Christian ethics, do X'. The mere imperative, 'do X', must however be a synthetically formal equivalent of the protasis, since merely *doing X* without further considerations may not in itself be in accord with Christian ethics. For example, if I give alms in order to become popular with the electorate, this is hardly Christian, as we understand it. One would have to say, 'do X privately, without trying for praise, fame, glory, etc.'. Here the qualifications involved are what make the apodosis formally equivalent to the protasis. Only with these qualifications can one formulate C as an equivalence, but it will read something like this: 'If you want to follow Christian ethics (which enjoins modesty, humility, self-effacement, etc.), then do X with modesty, humility, self-effacement, etc.' However, such an imperative is very impractical, because it is tautological. It says little more than, 'If you want to follow Christian ethics, then do X according to Christian ethics', which is no help at all.

Thus the lack of a complete distinction has led Harsanyi into a difficulty. Since we must grant that formal hypotheses can be either of form B (analytic) or form C (synthetic), we may now ask whether either can do the job Harsanyi wants them to do. He says, 'under the present theory the highest order moral rules become *analytical* statements where the apodosis only enjoins what is *entailed* by the formal requirements in the protasis' (p. 314. My italics). But what form do such rules take? Not those of form B (which are analogous to game rules). In our example—Harsanyi's own highest-order rule will be considered below—the rule, 'If you want to follow Christian ethics, do X', as it stands is *not* an equivalence, as we have seen. To make it such, one must do one of two things: (1) he must so qualify 'do X' that the rule becomes little more than a tautology (as pointed out above); or (2) he must make the apodosis a *complete disjunction*. Thus, 'If you want to follow Christian ethics, do A or B or C or D, etc.'. Such a statement is again useless as a moral rule. One does not give a list of all possible actions in giving a moral rule, but specifies one or the other in particular, *viz.* 'do X'. But if we take any one of the disjunctions alone, it is always arguable. It does not *necessarily* fulfil the formal criteria expressed in the apodosis: *i.e.* it is synthetic. But if this be the case, then moral rules as hypothetical imperatives are either (*a*) not analytic at all, but are always synthetic; or (*b*) if analytic, tell us nothing, being tautological or uselessly multiple. Harsanyi cannot be satisfied with the latter case. He is left only with synthetic statements.

The failure to observe the above distinction infects his theory as to the highest-order formal criterion, namely, the 'true' preferences of the individual addressee. Harsanyi arrives at this criterion by way of a sympathetic observer. But since the 'true' preferences of the addressee are the basis for any sympathetic observer's recommendations, then they must be the highest order moral criterion. Thus a highest-order moral hypothetical imperative would be something like this: 'If you want to act according to your "true" preferences, then do X.' Is this statement analytic, as Harsanyi explicitly maintains? Not at all. No matter what my 'true' preferences, it can always be argued whether X will satisfy them or not, whether X is the right or moral thing to do, etc. In a word, the statement is synthetic. Further, X is only one way of acting according to my 'true' preferences. There are others. But what is the *best* way? If we want to answer this question, which certainly seems important in ethics, we have to look to other things in order to answer it, things like circumstances, individual personalities, social customs, religious beliefs, etc. We need these anyway in order to determine our 'true' preferences. But even assuming that we could answer the question (and it would certainly be synthetically answered), we can see that an answer could never be given by a hypothetical imperative-moral rule.

Thus Harsanyi's attempt to establish an ethics in terms of analytical hypothetical imperatives fails. Whether all moral rules are in fact hypothetical imperatives is another point entirely, with which I do not wish to argue here. But if they are, they are certainly not analytic.

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ARE MORAL ARGUMENTS ALWAYS LIABLE TO BREAK DOWN?

In her stimulating article 'Moral Arguments' in MIND, lxvii (1958) 502 ff., Mrs. Philippa Foot has argued that factual premisses can entail evaluative conclusions, and therefore that the thesis that moral arguments may always break down must be rejected. I shall try to show here that the premiss of Mrs. Foot's own argument does not entail her conclusion. It would be quite consistent to accept her contention (the truth of which I shall not discuss) that factual premisses can entail evaluative conclusions, and yet insist that moral arguments may always 'break down', in the sense that no one need accept them unless he happens to hold particular moral views.

A certain French general in Algeria was reported in *The Times* on 2 June, 1958, to have come to the conclusion, after searching his conscience and consulting his spiritual advisers, that the torture of his arrested suspects was morally permissible whenever something was likely to be found out that would enable him to prevent further loss of life. We may suppose his reasoning, reconstructed in terms of Mrs. Foot's model, to have rested on an analytic truth that if an act is likely to lead to the saving of many lives, without being likely to destroy any, it is morally permissible. But the general has nevertheless had many critics. We may suppose their reasoning, similarly reconstructed, to have rested on an analytic truth that if an act consists in the wilful infliction of pain on those who have not yet been found guilty of any offence by due process of enquiry, it is morally impermissible. And it seems that any argument in the sense of 'reasoned controversy' between the officer and his critics might conceivably break down at the point where each side recognised the issue to depend on which of two analytic truths should be chosen as a basis for reasoning about the matter in hand. Of course, the argument might produce much more subtle argumentation on either side. But at some point it would almost certainly break down, if *The Times*' report is correct, and the analytic truths to be applied would be differently chosen. Indeed, the situation seems at least partially analogous to what it would be if the argument were reconstructed according to one of the models favoured by Hume, Stevenson, Ayer, or Hare and criticised by Mrs. Foot. It is just that the choice is now between two or more analytic moral principles, instead of between two or more non-analytic moral principles.

It might be objected that this is altogether too paradoxical. One might well expect, by employing the concept of analytic truth and basing one's concept of a principle of moral inference on it, to be able to construct a model of moral argument in which acceptance of agreed truths as premisses necessitates acceptance of just such and such a conclusion. If it is analytic that all men who have gone through no ceremony of marriage are bachelors, and one agrees that

George has gone through no ceremony of marriage, one feels one cannot help concluding that George is a bachelor. But what about the marriage by repute still recognised by the courts of some countries? Again, Mrs. Foot and I probably have much the same concept of a chair. But that does not ensure our agreement in every case whether some particular object is a chair or not, only in a great majority of cases. With sufficient ingenuity one can apparently think up borderline cases for any given analytic statement, if what the statement purports to do is to analyse a concept in everyday use. For if people cannot foresee all possible borderline cases they cannot anticipate all such cases in moulding their everyday concepts. They cannot determine in advance whether each such case is to fall under the concept or not. Hence Peirce (*Coll. Papers* 6. 496) and Russell (*Australasian Journal of Philosophy*, i (1923), 84 ff.) attributed 'vagueness', and Waissmann 'open texture' (*Proc. Arist. Soc.*, Supp. Vol. xix (1945), 119 ff.), to all ordinary concepts. In short, Mrs. Foot's analytic model turns out to expose moral arguments to breakdown at one point at which any non-analytic model so exposes them, namely where a single concrete situation exhibits features that bring it under one moral generalisation valuing it favourably and under another valuing it unfavourably and both parties to the controversy profess to accept both generalisations in normal circumstances.

Of course, a man can always meet borderline cases by a redefinition of terms. He can choose which, for him, are the more important criteria for applying a term, and which are the criteria that, in a certain kind of tight squeeze, should be disregarded. But once he has made his redefinition his relevant analytic statements will now be analytic of the comparatively sharp-edged concepts that function in his own statements, not of the more fuzzy-edged ones that otherwise he would have to share with his fellows. So we might suppose the French general and his critics to redefine their own several conceptions of moral permissibility in order to avoid these conceptions' having contradictory consequences in a particular case and producing a conflict of duties. On the one side a prospect of preventing further loss of life would come to make an exception to the general impermissibility of torturing suspects. On the other side the use of torture would appear as an exception to the general permissibility of doing what is likely to save life. But though the premisses of each side's argument may still entail its conclusion, the entailments no longer rest on concepts in common use by both parties to the controversy. So, though the formal contradiction has been avoided, it has been avoided at the cost of splitting the erstwhile common concept of moral permissibility into two different but sharper ones. And the direction in which a controversialist chooses to sharpen his concepts in such a situation is as much a matter for choice, or for reference to further principles about which similar borderline cases may arise, as is the analogous situation in a non-analytic model of moral controversy.

Admittedly, the analytic model has one advantage in forestalling breakdowns of moral argument. It excludes the existence of cases in which a disputed act is brought by one party under a moral principle of very wide generality which the other party totally rejects. But it is doubtful whether the school of thought that Mrs. Foot is attacking would be well advised to place much reliance on the existence of such cases. Even if moral principles are not analytic people are not going to argue much with one another about moral issues, whether in private, in committee, in correspondence columns, or in parliaments, unless they have a wide measure of agreement about general principles. It would be sheer waste of time. The strength of the position Mrs. Foot is attacking lies in the fact that a moral argument can break down even between parties that share the same general principles if they disagree about the moral importance of these principles relative to one another. But this fact is logically independent of whether these principles are correctly construed on the analytic model or on some non-analytic one. So Mrs. Foot's spirited defence of the former model cannot achieve anything towards disproving the thesis that moral arguments may always break down.

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MORAL ARGUMENTS

MRS. P. FOOT, in her article "Moral Arguments" (MIND, lxvii) seems to make two separate points without differentiating them clearly from one another.

The first one is logical. Moral argument presupposes agreement concerning the criteria of goodness, rightness or obligatoriness. Apart from such agreement, it could not (logically) begin. This point is indisputable, and no denial of it is implied in the non-naturalist position. Before moral argument can commence the disputants must either both be committed to the same moral principle or principles, or one of them, for the sake of the argument, must be prepared to accept the principle or principles of the other as a starting-point. The latter alternative is not uncommon: a teetotaller, for instance, may try to reform a hard-drinking hedonist by showing him that excessive drinking leads ultimately to more misery than pleasure. Only when there is agreement about the criteria can the disputants proceed to adduce evidence to show that a proposed act will, or will not, fulfil the said criteria and so be good, right or obligatory, or not so.

The other point is factual. Mrs. Foot suggests that there are rules for the use of 'good', 'right', 'ought', in the same way that there are rules for the use of 'rude'. A rude act is, by definition, one which "causes offence by indicating lack of respect" (p. 507). 'Rude' carries this precise and universally agreed meaning in our language, and if one does not use it in accordance with this rule, one will talk palpable nonsense: if, for instance, one says, "A man is rude when he behaves conventionally" (p. 508). But have moral terms similarly precise definitions in our language? The question, "Is offensive conduct rude?" is self-answering. We should refer the questioner to a dictionary. If he persisted: "Yes, I know that offensive conduct is the lexical definition of 'rude', but is it really rude?", we should have no idea how to answer him. There are no other criteria for the use of 'rude'. In reply to this question nothing can be said, and so it is meaningless. But the case is far different with specifically moral terms. "Is X conduct right?", where X is any naturalistic description and 'right' is used in a moral sense, is not self-answering. We should not refer this questioner to the dictionary. We might, of course, point out to him that, in our society, such conduct has generally been regarded as right, or even universally, if such were the case. But if he persisted, "Yes, I know that, but is it really right?", the question would not leave us at a loss for anything to say. People have asked this question of generally accepted definitions of rightness and other people have attempted to answer them. In order to answer, one must, of course, secure the questioner's agreement to some criteria for the use of 'right'. If no such agreement can be reached, then the question must go unanswered. But there are no particular criteria for the use of ' right ', such that we can say, " Unless a man uses the word in accordance with these, he is talking nonsense ". The point is not so much that moral terms cannot be defined in naturalistic terms, as that they are not. There is an ' open ' character in their use. In moral argument it is always the case that the disputants are agreed upon certain criteria ; but it is never the case that, if they had agreed upon any other criteria, they would necessarily be talking nonsense. The final appeal in moral argument is always to the criteria to which the disputants are committed ; but this commitment arises from their decision, it is not implicit in the meaning of moral terms.

Moral terms do not so much have a meaning as do a job. They express options of a peculiar kind. If they were defined in naturalistic terms, we should have no use for them. Moral discourse could then give place to talk about psychology, *e.g.* what maximises happiness, natural science, *e.g.* what conduces to evolution, etc. Bentham and others have wished this to happen ; but so far it has not happened.

W. D. HUDSON

ABOUT "*aRb*"

In the issue for April 1958 there is an article by Mr. Irving M. Copi on Wittgenstein's *Tractatus*. In the course of his argument he refers to my discussion note on paragraph 3.1432 of the *Tractatus*, which appeared in MIND for April 1955. I had assumed in this note, as Mrs. Daitz, to whom I was replying, had assumed in her article, and as Russell had assumed in his *Introduction* to the *Tractatus*, that 3.1432 was about the proper analysis of ordinary-language sentences of the form "*aRb*". Mr. Copi, however, questions this assumption, as part of his general argument that the *Tractatus* is not about ordinary language. The paragraph is as follows:

> 3.1432. We must not say, "The complex sign '*aRb*' says '*a* stands in relation *R* to *b*'"; but we must say, "*That* '*a*' stands in a certain relation to '*b*' says *that aRb*".

Now Mr. Copi maintains on page 155 that this should be taken as forbidding us to use the locution "The complex sign '*aRb*' says '*a* stands in relation *R* to *b*'" *of an adequate notation*. However, it follows from Wittgenstein's footnote to the first page of his exposition, that 3.1432 is intended as a comment on

> 3.143. That the propositional sign is a fact is concealed by the ordinary form of expression, written or printed.
> (For in the printed proposition, for example, the sign of a proposition (*Satzzeichen*) does not appear essentially different from a word. Thus it was possible for Frege to call the proposition a compounded name.)

This, quite obviously, is about ordinary language, and makes the general point that printed propositions look like words or names, this resemblance concealing the true state of affairs, which is that they are *facts*. What Wittgenstein means by calling them facts is explained in the paragraph on which 3.143 is itself a comment, *i.e.*

> 3.14. The propositional sign consists in the fact (*besteht darin*) that its elements, the words, are combined in it in a definite way. The propositional sign is a fact.

This definite way, moreover, expresses the sense of the proposition. (We learn this in 3.1431.) It seems to me, then, quite clear that Wittgenstein intends 3.1432 as a particular exemplification of the general point in 3.143. He has implied that the complete analysis of a propositional sign must show that it is not a mere word or name, but a fact, that is, a definite combination or ordering of terms. Thus we must not say of an "*aRb*" proposition for instance, that it says, that *a* stands in *R* to *b*, because this analysis merely repeats the sequence of terms in the analysed sentence. Such an analysis does not show what it is about "*aRb*" that expresses its sense. In order to show that, we must refer to the ordering of the terms in the sentence to be analysed. We must say "That '*a*' stands in a certain relation to '*b*' says that ***aRb***". Then we have shown that an ordering of terms conveys the sense of the proposition.

This interpretation appears to me quite natural and coherent No harm is done to the sequence of Wittgenstein's ideas. Mr. Copi's interpretation, on the other hand, represents the paragraph as unnecessarily enigmatic, and also involves damage to the scheme of exposition in the *Tractatus*. The concept of an adequate notation is introduced at 3.325, some thirty paragraphs later, openly and in as many words. It would have been strange if Wittgenstein had introduced it at 3 1432, where he is still concerned with the question of how propositional signs *do* signify, and not yet with the idea that there ought to be a notation which would exclude confusions as to how they do so.

How *does* "***aRb***" signify? On page 154 Mr. Copi wants to sentence me to Bradley's regress about relations of relations (*Appearance and Reality*, chapter iii). This is because I said in my note that to the order of the elements '*a*' '*R*' and '*b*' corresponds the structure of the elements *a*, *R* and *b*. However, I did not refer to *R* as a relation, and as for structure, I did not use only that expression, but kept on saying that the word-order showed *that aRb and not that bRa*, or *that aRb rather than that bRa.* Thus the word-order indicates that the fact *that aRb* is a kind of alternative. I'll try to explain what I mean by this.

"Telephone" is a word for a kind of object. Analogically, "*R*" could be a word for a kind of situation. For example : this book's being on this newspaper, this same book's being under this same newspaper, the kitten's being under the table, the dog's being under the table, the lamp's being above the table. These can be seen as situations of the same kind. There could be a word "*R*" for this kind of situation. Then "*R*" would be a sort of noun.

Though nouns are only words for *kinds* of object, we can use them to refer to *particular* objects. We may do so by accompanying them by other words that make the reference precise. These other words help to specify *which* object of that kind is meant. For example : the *new* telephone, the *black* telephone, the *Kruschev* proposal, the *goods* train. Notice that we have names and nouns here as well as adjectives.

Any familiar object has several features that could be mentioned in this specifying way. But, given its kind, there are typically just two features of a situation that are outstandingly important : the identity of the objects involved in it. A marriage has been solemnised, but who got married?

I interpret, then, "***aR***" as a reference, in which "*R*" shows the kind of situation, and "*a*" specifies. For example : in "Tom loves Mary", "loves" shows which kind of situation is meant : "Tom" specifies, so that the reference is, to a love-situation involving Tom.

Possible features of an object may be asserted *of* it after it has been referred to. To assert such features, we mention them in a predicate. For example : the black telephone is *broken* : the broken telephone is *black*. Well then, if one such feature of a situation has been used to specify, why not any other such possible feature to assert? I use " Tom loves " to specify a love-situation involving Tom. The word-order shows that " Tom " is a specifier. I add " Mary " to say that the other member of the situation is Mary. So " Mary " is like a predicate. The word-order shows that it is.

So " Tom loves Mary " says at least this : a love-situation involving Tom, involves Mary. But this is a symmetrical assertion. I might just as well have specified with " Mary " and asserted with " Tom " : I would have said the same. And all " *aRb* " sentences interpreted by this model become, in the first instance, symmetrical assertions. But consider the following analogy.

Sometimes we invert the subject-copula-predicate order for rhetorical effect, thus : calm is the sea. We can say, here are two syntactic patterns available for making assertions in English. Other patterns are available for other things : *e.g.* copula-subject-predicate gives a question : Is the sea calm?

Imagine a language that has, on the whole, the same patterns, parts of speech, and words as English, except for this : word-order also has a *semantic* function when it comes to colour-adjectives. These people perceive just the same colours as we do, but their language distinguishes each colour into a lighter and darker shade. Their colour-words mean the lighter shade when they precede their nouns, and the darker shade when they follow them. Thus " red is the book " means that the book is light red, and " the book is red " means that the book is dark red. This is learned ostensively, from childhood. There is no question of colour-words signifying in any other way.

Then although the word " red " in isolation—*e.g.* in a dictionary, or chalked up on the wall—could be said to mean just what the English word " red " means, a speaker of this language could not just call something red. The order-convention would force him to assert the lighter or darker shade, since the adjective would have to precede or follow the noun by means of which he referred to what he wanted to characterise.

I see the kitten on the table. I put it under the table. I see that these situations are of the same kind. My seeing this is like a speaker of that language, who has watched the sunset turn from light to dark red, recognising that it is still the same colour. Let there be a word " ϕ " for the kind of situation I recognise. Suppose I say " the kitten ϕ the table ". By our previous analysis this says that kitten and table are in this kind of situation, and no more. We do not have such a word in English. If we had, it would be analysed as the name of a symmetrical relation—because it wouldn't matter whether the kitten was under the table or the table under the kitten : both these situations would be covered. It wouldn't matter whether I said " the kitten ϕ the table " or " the table ϕ the kitten ".

We don't want to know just that the kitten ϕ the table. (*Cf.* a speaker of that language: we don't want to know just that something is red.) We want to know which of this pair of situations, covered by "ϕ", is the case. (We want to know whether it is light or dark red.)

Let there be another word, "ψ", which is also for the kind of situation here. So far, then, it is synonymous with "ϕ". But, unlike "ϕ", let it work with a semantic order-convention. The order of words is to say which of the pair of situations holds. Thus, let us say, the thing mentioned before "ψ" is to be the upper one. (This is ostensively learned.) And now if I say "the kitten ψ the table", I have said that the kitten is on the table, because by using "ψ" instead of "ϕ" I not only indicate the *kind* of situation of which the kitten is a member, but also *how* it is a member of that kind of situation: by putting "kitten" first rather than "table", I show that it is the kitten that is *on*. This is a theory of the word "on".

And if no word "ϕ" exists, but only "ψ" exists, I *cannot do otherwise* than express one of these paired situations. I *cannot say*, in this language, that kitten and table are in the kind of situation that "ψ" means, and leave it at that.

The suggestion is, then, that symmetrical relation-words are like "ϕ", and that non-symmetrical (including asymmetrical) relation-words are like "ψ". The semantic function of the word-order for each "ψ" is learned together with the meaning of each "ψ". The purely syntactic function of the "*aRb*" word-order is of course the same whether the "*R*" is like "ϕ" or like "ψ": it shows what word specifies, and what word asserts.

The so-called converse of an "*R*" now appears as the nearest available synonym for "*R*" that works with the reverse convention of order. *E.g.* "below" is more or less synonymous with "above" in that it is for the same kind of situation on the whole: but in "*aRb*" sentences the thing mentioned before "above" is the upper thing, and the thing mentioned before "below" is the lower. Thus "*aRb*" = "*b Rc a*". Of course, to have a "converse", the "*R*" must be a ψ-word, not a ϕ-word. Thus it is that the converse of a symmetrical relation is said to be identical with that relation.

If all this is right, talking about *e.g. the relation* "greater than" is like talking about The Telephone. Talk of the *sense* or *direction* of a relation is explicable in terms of a failure to distinguish between semantic and syntactic word-order. "Relation" itself vanishes as a term of the analysis, and the mode of assertion in "*aRb*" becomes comparable with the subject-predicate mode after all.

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STRAWSON ON REFERRING

In his paper "On Referring",[1] P. F. Strawson argues that what Russell gave as an analysis of propositions involving definite descriptions is actually not part of what is asserted in these propositions, but rather part of what they imply in a special sense of 'imply'. This sense of 'imply' is what Strawson also calls presupposing; it is that p implies or presupposes q when p would be neither true nor false, the question of its truth or falsity would not arise, unless q were true. (*Cf.* p. 34 and Strawson's "A Reply to Mr. Sellars", *The Philosophical-Review*, lxiii (1954), p. 216.) Thus Strawson says, for example, that the referring use of a definite description implies, in this sense, "that we are, or intend to be, referring to one particular individual of the species 'such-and-such'", *i.e.* of the sort indicated by the expression following the word 'the' (p. 36). In other words, as he says, Strawson takes such a use of a definite description to imply that "the existential conditions described by Russell are fulfilled" (*ibid.*).

In the original version of the paper, Strawson also held that one can intend to refer to or to mention something but fail to do so. He talks about "spurious uses" of uniquely referring expressions; he says, for example,

> . . . when we utter the sentence without in fact mentioning anybody by the use of the phrase, 'The King of France', the sentence does not cease to be significant: we simply *fail* to say anything true or false because we simply fail to mention anybody by this particular use of that perfectly significant phrase. It is, if you like, a spurious use of the sentence, and a spurious use of the expression . . . (p. 35; Strawson's italics).

Strawson also talks about "succeeding in mentioning somebody or something" (p. 36). However, to the reprinted paper Strawson added some footnotes indicating that there was an error in this. After discussing make-believe or fictional uses of uniquely referring expressions, he concludes,

> Hence we can, using significant expressions, pretend to refer, in make-believe or fiction, or mistakenly think that we are referring when we are not referring to anything (p. 40).

To this he now attaches as a qualification the following footnote:

> This sentence now seems to me objectionable in a number of ways, notably because of an unexplicitly restrictive use of the word 'refer'. It could be more exactly phrased as follows: 'Hence we can, using significant expressions, refer in secondary ways, as in make-believe or in fiction, or mistakenly think we are referring to something in the

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[1] Originally in MIND, N.S., lix (1950), pp. 320-344; reprinted, with additional footnotes, in *Essays in Conceptual Analysis*, ed. Antony Flew (1956), pp. 21-52. All subsequent references are to the latter, unless otherwise indicated.

primary way when we are not, in that way, referring to anything ' (*ibid.*).

What I wish to suggest in this paper is that both of Strawson's accounts have gone wrong because he has made too little change in Russell's theory regarded as an account of the ordinary use of this sort of referring expression. The defects of Strawson's account are, first, that the ordinary distinction between failing to refer to or to mention something and referring to or mentioning something is not the distinction that Strawson draws, and, second, that the suggestion of ordinary usage is that referring is the same sort of thing whether the thing referred to exists or does not exist or is fictional or make-believe or whatever else it may be. Finally, I will offer some suggestions toward an account which would avoid the difficulties caused by regarding it as important to the question of whether or not something was referred to or referred to in some primary way, whether or not the thing referred to exists.

The first defect of Strawson's account in its original version is that he thought that it was possible to intend to refer to or to mention something and, presumably, although he does not actually use this phrase, to try to refer to something, and then either to fail or succeed in referring to or in mentioning that something. Now I think it is possible to do these things, *i.e.* these phrases do have a use, but that the ordinary use of these phrases is not the use that Strawson suggests that they have. Thus the only way in which one can fail to refer to or to mention something is either through (1) omitting to refer to or to mention the thing at all, or (2) mis-speaking, being interrupted, or not getting the words out. In the first case, one has not even tried to refer to or to mention the thing in question and there is no correlative success in mentioning or referring to the thing : what is opposed to failing to mention something is actually mentioning it or remembering to mention it or coming near to mentioning it without actually doing so, etc. But as Strawson originally viewed the matter, one could fail to refer to something by using some expression which could be used to refer to that thing if it existed but which failed to do so, since that thing actually did not exist, although one thought that it did. Now this situation sometimes obtains, but it is worth nothing that when it does, we do not say that the person has failed to refer but rather we point out to the person that the thing does not exist. In the second case, one can fail to refer to or to mention something, but not because the thing does not exist, although one thought it did ; rather, one fails because one does not even succeed in uttering the referring expression, a minimal condition for using it to refer to something. Here the existence of the thing one would have referred to is obviously irrelevant.

Now it is clear that these ordinary meanings of the phrase ' fail to refer ' are not what Strawson has in mind. The usual phrase for what he does have in mind is ' referring to something that does not exist, thinking it does '. This he would express in his terminology as 'intending to refer to something, but failing to do so' or as 'mistakenly thinking that one is referring when one is not referring to anything'. In the revised version of his account, he would express this by 'mistakenly thinking one is referring to something in the primary way when one is not, in that way, referring to anything'. But clearly this would differ from the ordinary phrase in the same way: if one says 'The King of France is bald', one is not corrected by the remark that one is not referring to anything but by the remark that there is no such person. In other words, Strawson's revised account still seems to imply that one can think that one is referring in the primary way to something when one is not. However, as 'refer' is ordinarily used, it seems that all one can do is to refer to something thinking it exists when it does not: but this nowise commits us to saying that one has not referred at all and it even looks as though one must have, if one referred to something which did not exist.

In connection with this, it may be noted that we would ordinarily talk about someone's *succeeding* in referring to something only if he managed to do so despite some obstacle or hindrance. But this again is independent of whether or not the person thinks the thing he refers to exists and of whether or not it does exist.

The feature of our ordinary use of 'refer' which Strawson seems not to have noticed and which he has certainly omitted from his account or from his use of 'refer' is that referring is ordinarily regarded as the same whether or not the thing referred to exists, is fictional, pretended, dreamt, or whatever. This can be seen from the fact that whenever any substantive is used to refer to something, it may be unclear what thing is referred to and a question intended to remove the ambiguity can be phrased with the word 'refer'. Ambiguous uses of substantives can occur in straightforward talk about things known or thought to exist, in jokes, in overtly fictional accounts, in relations of dreams, and indeed anywhere. And the question 'Which one are you referring to?' and similar questions involving the word 'refer' can be and are asked in all such cases. The only possible inference, it seems to me, is that there is no necessary connection between the existence of what is referred to and one's referring to it. That is, referring is the same sort of linguistic performance in all these cases.

This is not to say, of course, that whether 'John loves Mary' is said on the street, in a joke, in a play, in a novel, etc. there will be no difference in the implications that it has regarding the existence of John and Mary. It is only to say that these differences in context of utterance and in the corresponding implications do not affect the question of whether or not John and Mary were referred to. If the sentence was used to make a statement or report and perhaps in some cases even if such a sentence was taken as an example, the substantives can fail to identify the persons referred to and the question of which John or Mary was referred to will arise. Indeed, one can even ask to whom a fictional character was referring when he used a certain substantive in a fictional conversation. In other words, not even the substantive which is used as the subject of the verb 'to refer' itself need refer to something which exists.

There is obviously raised here the question of whether Strawson's account could be revised in order to accord more closely with the ordinary use of 'refer' which is in question in these examples. I think it probably could and that this might be done along lines some of which are adumbrated by Strawson. In discussing the uniqueness condition of the non-fictional use of uniquely referring phrases, Strawson suggests that the presupposition of uniqueness should be stated in terms of referring, *i.e.* that the notion of referring should be used in stating the presupposition itself. In connection with distinguishing what is asserted from what is implied by a uniquely referring use, and referring to the use of the phrase 'the table' in the sentence 'The table is covered with books', Strawson says,

> It is . . . tautologically true that, in such a use, the phrase will have application only in the event of there being one table and no more *which is being referred to*. . . . To use the sentence is not to assert, but it is (in the special sense discussed) to imply, that there is only one thing which is *both* of the kind specified (*i.e.* a table) *and is being referred to* by the speaker. (p. 37 ; Strawson's italics).

Now in the discussion of the presuppositions of a uniquely referring use of an expression given earlier in the article, there is no mention of the presupposition that there is only one thing which is both of the kind specified and is being referred to ; but it is said there that what is presupposed is that there is one and only one thing of the kind specified. But, as Strawson states in this later passage, it is obviously the former fact which is presupposed.

Now if we were to use 'refer' in the ordinary way, then we would get, I believe, an account of these presuppositions of the use of uniquely referring expressions which is what Strawson ought to have said, but without his altered use of 'refer' and without the difficulties about the existence of what is referred to. Thus anyone who asserts that the King of France is bald, using the sentence 'The King of France is bald', will be said to have referred to the King of France, as indeed he would ordinarily be said to have.

Then a rough sketch of what the relations between referring and existence are would be the following. To begin with, our ordinary language can be and is used to discuss real people, things, and occurrences, but it is also used to discuss the people, things, and occurrences in novels and plays and in jokes and dreams. The same language can be and is used in discussing all of these—exactly the same expressions—so long as it is understood by everyone concerned what sort of thing, *e.g.* real people, people in a novel, or people in a dream, is being discussed. When what sort of thing is being discussed is *not* understood, an indefiniteness or ambiguity in the use of all the language prevails and it is impolite or something verging on lying not to inform a listener who is not aware of or does not understand what the subject-matter is. Such information is regularly given when it is thought that someone may not understand and there are many ways of giving it. In some cases of discourse shifted from the straightforward employment special sorts of language are used, *e.g.* the historical present tense in the verbs of stage directions ; but in other cases the same language is used, *e.g.* the imperative mood of verbs in recipes and other sorts of directions. The difficulties in connection with the relation between referring and existence seem to me to be caused mainly by the fact that when discussing other sorts of thing than the real world we use largely the same language and that we use it in closely similar ways.

Now as the discourse shifts, the implications of the utterances may and usually do change, though the expressions uttered may remain the same. These implications shift systematically, so that by finding out the sort of discourse that is occurring one is able to take the utterances as they ought to be taken, *i.e.* as they are meant. Thus when we find out, for example, that it was in a dream, in a novel, in a story or lie told, or in a fairy tale that a certain person received a large sum of money, we would not seek to verify the fact by checking with his bank. But the same expression, ' He was given a large sum of money ', say, may be used in all these cases.

One sort of implication that changes is, of course, the implications of the use of referring expressions. This fact is no doubt what Strawson has in mind when he speaks of the distinction between primary and secondary uses of referring expressions, which in my terms would be the distinction between the use of referring expressions in straightforward discourse about the real world and their use in shifted discourse about fictional, dreamt, lied-about, fairy persons and things, as well as those in directions, recipes, jokes, and the like. But it is a noteworthy fact that the phrases 'to refer to', 'to mean', 'there is', 'there are', 'to exist', etc., are used in all sorts of discourse and the implications of their use and of the use of referring expressions in connection with them change just as do those of any other expressions. That is, whether it is the real world, a dream, a joke, a novel, directions, or a recipe that is under discussion, we are prepared to and do talk about what there is, whether there is a thing of a certain kind or a person called so-and-so, that someone, real or not, was referring to, etc. For example, the implications of the use of the referring phrase ' the table ' are the same in ' I was referring to the table in the corner ' as they are in ' The table is covered with books ', whatever the sort of discourse is in which both are uttered. If it is understood in the one case that no particular table is being referred to, *e.g.* in discussing the properties for a play to be performed, this would also be understood in the other—provided that it is clear what sort of discourse is being conducted.

What I wish to suggest, then, is that the locus of the problem about the relation between the use of referring expressions, whether uniquely referring expressions or some other kind, and the existence of the things referred to—where this problem lies is not in the account of referring, if the ordinary use of the verb ' to refer ' is to be any words, not even the substantive which is used as the subject of the verb 'to refer' itself need refer to something which exists.

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What I wish to suggest, then, is that the locus of the problem about the relation between the use of referring expressions, whether uniquely referring expressions or some other kind, and the existence of the things referred to—where this problem lies is not in the account of referring, if the ordinary use of the verb ' to refer ' is to be any guide, but rather in that of the shifting of discourse from the normal straightforward sort to discourse about things dreamt, joked about, in novels, etc. That is, to take the case of unique reference, if one uses the sentence ' The table is covered with books ', it is always presupposed that there is one table that one is referring to—though it may be a table so far unspecified which is to be used on stage, an imagined table, etc., as well as some real table. In short, the use of ' to refer ' and ' to exist ' and similar expressions changes just as the use of other expressions changes when the sort of discourse which is being conducted shifts. If, then, the ordinary use of these expressions is to be the guide, the account of the connection between the use of these expressions should be given in terms independent of what sort of discourse they are employed in—for this connection *is* independent of the latter, as is shown by the way we ordinarily employ ' refer ' and ' there is '.

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KEEPING

Professor Ryle has said (*The Concept of Mind*, pp. 149 f.) that the verbs in phrases such as 'keeping a secret', 'holding [or 'keeping'] the enemy at bay', 'retaining [or 'keeping'] the lead', are achievement verbs which signify 'more or less protracted proceedings'. Mr. F. N. Sibley (Mind, lxiv. 467 ff.) calls these 'retention' verbs and argues, against Ryle, that they signify tasks or activities and not achievements. Now since 'keep'—and certain variations on it like 'adhere', 'continue', 'maintain', 'remain', 'retain', 'stay'—is a verb which appears in a huge host of English phrases, including many which are used to speak of the mind, it is worth arguing that both these views are mistaken. I shall try to do this (*a*) by suggesting another view, (*b*) by supporting certain of Sibley's objections to Ryle and raising others, (*c*) by criticising Sibley's own view.

(*a*) We seem to use the verb 'keep' with many kinds of other words, whether signifying *activities, e.g.* keep (on) talking, moving, smoking, watching; or *achievements, e.g.* keep a hold of, keep the lead, keep in sight, keep in mind, keep in the place you've got to; or *tendencies, e.g.* he kept (on) smoking after his doctor had warned him to cut out cigarettes (contrast 'he kept (on) smoking after I had told him to put the cigarette out'); or *abilities, e.g.* he kept his flair for snooker; or *feelings, e.g.* I keep feeling cold; or *qualities, e.g.* he kept his good looks, his boyish spirits. No doubt there are others. In many of these we can substitute, with linguistic changes, 'continue', 'remain', 'retain', etc.

The function of 'keep' in all these seems to be to suggest that whatever was so before is still so, remains so, continues so, does not stop or cease being so. The man who keeps smoking despite my order engages in the activity in which he engaged before; the man who keeps smoking despite his doctor's warning remains with the same habit as before. The firm which keeps its acquired or achieved reputation still has it; and the runner who has taken the lead remains ahead. The man who keeps the promise he made still binds himself.

This prolongation force of 'keep' is strong also in what may be its basic uses such as 'keep (or store) apples', 'keep (or be occupied with) bees, house', 'keep (or own or provide for) a large body of servants, a mistress'.

In the same way 'keep' has the force of what I shall call a double negative to suggest that whatever we were speaking of, whether activity or achievement or disposition or feeling or quality, has not lapsed, stopped, ceased, vanished, got lost. He who keeps talking does not cease talking; he who keeps silence (or silent) does not start talking; he who keeps still does not move, he who keeps moving does not stop. He who keeps the lead does not lose it; he who keeps to the nearside lane does not venture out. He who keeps coming regularly at noon does not give up the habit. To bear in mind is not-to-forget; to keep hold is not-to-let-go; to keep faith is not-to-break it; to keep a secret is not-to-reveal it; to keep one's honour is not-to-lose it. This double negative force is equally apparent where 'keep' is used with a negative word, *e.g.* he kept missing the target, forgetting the names, losing his way, dropping the parcels. That is, he did not cease to do what he had been doing.

(*b*) I agree with Sibley that Ryle did not distinguish phrases signifying achievements which come gradually—such as 'slowly reaching a position of eminence'—from phrases signifying retentions—such as 'retaining the lead'. Sibley thinks the reason for Ryle's mistake here is that both achievements and retentions are incapable of failure; but it seems to me that a more likely reason is that Ryle treated only those cases where 'keep' occurs in an achievement context. The success of the man who keeps the enemy at bay or who retains the lead was to stop the enemy or to take the lead; and this success continues. Verbs of achievement, success, or arrival signify the getting to a certain position, result, terminus, usually after an effort towards it. Retention verbs used in such contexts signify the staying in, remaining in, continuing in, the not losing, that position, result, or terminus.

It is worth pointing out here that we distinguish between a continuous series of successes of the same kind and one continuous success by saying, for example, 'he kept taking the lead' and 'he kept the lead (*i.e.* he took it and kept it)'. Of course, in the ordinary, non-technical or non-Rylean, sense of 'achievement', we often do call 'protracted proceedings', whether performances or tendencies or successes, achievements, and reward or congratulate people accordingly. To keep talking for several hours, to remain patient despite vexatious delays, to keep faith over the years, to keep the lead for half the race, may all be quite remarkable achievements. Patience and perseverance made a bishop of His Reverence!

(*c*) Sibley seems inclined to believe that because retention verbs do not signify achievements, they signify tasks or activities. Now I do not quarrel with those of his arguments which are aimed only to show that retentions are not achievements—at least in the sense of 'arrivals' or 'gettings'; but his positive arguments that 'retaining' (or 'keeping') is an activity verb seem to me mistaken.

He is mainly concerned to show that often a retention requires effort and activity, and requires these in another way than achievement does; his mistake is to conclude that retention is itself an effort or activity. But though it may be related, as we shall now see, in various ways to activities, retention is not itself an activity. If one is engaged in an activity, *e.g.* talking or smoking, then to keep talking or smoking requires more of these activities, because it means doing more of them. If one has attained a certain result, *e.g.* gained a reputation or a lead, then the activities required to keep that success are usually the same as those required to attain it. A reputation gained by hard, honest, and brilliant work is usually kept by the same methods; a lead which required the straining of every nerve to capture may, or may not, require the straining of every nerve to hold. Resting on one's laurels or on one's oars is notoriously dangerous. Fortunately, however, it often happens that no effort is required to maintain what it needed a lot of effort to acquire. We often keep our reputations or our lead because of the weakness of our rivals. But even where it requires effort and activity to keep what we have got, the keeping is not itself these efforts or activities. It may require anything from bribery to coercion to retain the services of my butler, but retaining his services is not itself bribing or coercing him. Keeping the lead is not itself the running fast which it requires as much as did the capture of the lead.

One man may have to indulge in practice to retain his abilities while his more fortunate colleague preserves them without effort; but retaining his ability to repeat the Hebrew alphabet is not itself any of the activities required to learn it or to retain it. Similarly, retaining a habit is neither an activity nor a habit. Indeed more effort may be required to break it than retain it. Certainly a man who retains the habit of smoking engages in the activity of smoking, but this is because the activity of smoking is the manifestation of the habit of smoking. Finally, though it often requires an effort to keep one's temper or one's health, what activities does the man engage in who retains his good looks or his boyish spirits?

Sibley rightly says that 'failure to retain would mean ceasing to retain'; but retention is always successful not because it is an achievement, as Ryle says, nor because it is a successful activity, as Sibley says, but because it is a not-ceasing. And it may be the not-ceasing of an activity or an achievement or a habit or an ability or a characteristic or something else.

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MR. WATKINS ON METAPHYSICS

In two recent papers, most interestingly and persuasively written,[1] Mr. Watkins argues (1) that metaphysical principles must on purely logical grounds be construed as factual propositions, *i.e.* as true or false, and (2) that in regarding them as regulative (directive, prescriptive, programmatic, etc.) one does not do justice to what might roughly be called " their connection with the world of brute fact ". In this note I should like to comment briefly on both these points.

1. Watkins's argument in support of his thesis that metaphysical principles are factual, even though they are not testable, is this: From

$$(y)\ \mathrm{R}(a, y) \qquad (A)$$

$$(\exists x)\ (y)\ \mathrm{R}(x, y) \qquad (B)$$

is deducible. A may be assumed to be testable and B to be untestable, in which case it is metaphysical. (This is so if, for example, ' R (x, y) ' is the relation ' x is a solvent of y ', where x ranges over all acids and y over all metals, and if a is a well identified acid, found in the National Physics Laboratory. The fact that the Laboratory houses more than one though a finite number of such acids may be ignored without loss of generality.) Since A is testable it is *a fortiori* factual; and since the untestable B is entailed by A it must be factual too.

Now *if* the number of individuals in the universe were *finite*, both A and B would be compound truth-functional statements. The quantifiers, which occur in them, would then be merely convenient abbreviations; and A and B would both—as is well known—be equivalent to finite conjunctions of finite alternations and equally to finite alternations of finite conjunctions. As regards the testability of A and B the only difference between them would be that there are more possibilities of falsifying A than there are of falsifying B. Whatever else Watkins's testable law-statements may be, they are certainly not compound truth-functional statements; and the occurrence in them of the universal quantifier is not a matter of mere convenience but of substance. His argument, therefore, depends on the assumption that the number of individuals in the universe is, in some sense of this term, infinite. Considered as factual this assumption would have to be formulated in terms of at least one existential and one universal quantifier—and would thus on Watkins's view be a metaphysical doctrine. (A general schema for its formulation would be: For every object . . . there exists a further object . . . such that . . .).

We must, therefore, conclude that his argument is not purely logical and, moreover, that in view of his purpose, which is to explain the logical status of metaphysical doctrines, it is circular.

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[1] Mind, vol. lxvii, no. 267. See also *Philosophy*, vol. xxxii, no. 121.

2. Watkins admits " that a person may reject a haunted-universe doctrine in its fact-stating form while subscribing to the prescriptive version of it "[1] and thus admits that *some* metaphysical propositions may be purely programmatic. He does not, however, seem to be clear about the connection between regulative principles for theory-construction and experience.

If one adopts a programme, which is neither true nor false, one usually believes that it is satisfiable. The scientist who adopts a programme for theory-construction, for example, believes that executing it will result in logically unobjectionable and empirically testable theories. Whether or not or how far this is so, depends not only on his programme but also on the world in which he finds himself. If we compare the truth or falsehood of factual propositions with the satisfiability or unsatisfiability of programmes, we note that whereas of two incompatible factual propositions one, at least, must be false, of two incompatible programmes one or both may be satisfiable or again unsatisfiable. It may be that of some programmes we shall never know whether they are or are not satisfiable. This, as Watkins would agree, does not make them meaningless.

The view that some metaphysical principles are programmes does not divorce them from experience. Moreover it does justice to the fact that a universe which is inhabited, perhaps not by ghosts, but by beings who think *and* act, has been and will continue to be " haunted " by programmes for the organization of society, for theory-construction and other activities. Lastly it does full justice I think to the influence of metaphysics.[2]

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[1] MIND, *loc. cit.* p. 356.

[2] These points are elaborated in chapters xxx-xxxiv of *Conceptual Thinking* (Cambridge 1955, Dover Publications, New York, 1958) and further examples are found in ' Philosophical Arguments in Physics ' in *Observation and Interpretation* (Butterworth, London, 1957).

VII.—CRITICAL NOTICE

Function, Purpose and Powers. Some Concepts in the Study of Individuals and Societies. By DOROTHY EMMET. London: Macmillan & Co. Ltd. (New York: St. Martin's Press), 1958. Pp. 300. 28s.

IN her Preface, Professor Emmet tells us that " this book has grown out of long-standing interests in social and political philosophy and in the philosophy of religion "—interests which she found being brought together and helped forward by reflections on some of the contemporary work in British social anthropology. She proposes therefore to discuss some of the concepts used by anthropologists in the hope that this discussion will " provide an indirect approach to a point of view on [certain] moral, political and religious questions ". Miss Emmet writes in a pleasant and urbane manner and no one could accuse her of failing in lucidity ; yet it is by no means easy to convey in a relatively short space what exactly are her ' findings '—for her book is diffuse and the point of view she is trying to express often elusive. Nevertheless, it must be said at once that in her critical examination of some of the concepts used by social anthropologists she says much that is of interest to more than one type of specialist.

In chapter I (Introduction) she draws attention to the fact that political philosophy and the philosophy of religion are " poor relations " among our studies, and that philosophers of religion " must be uneasily aware of the scarcity of constructive or even exploratory work which can stand up by critical and logical standards with contemporary work in other branches of philosophy ". Miss Emmet would probably not agree that the philosophy of religion requires such radical transformation as to render its claim to rank as an independent branch of philosophy highly disputable ; but she nevertheless recognizes that we cannot rehabilitate the language of the tradition in which it grew up, namely that of Thomist Natural Theology and Natural Law. She likewise recognizes, commendably enough, that in political philosophy we cannot rehabilitate the language of Philosophical Idealism.

Miss Emmet then makes some preliminary remarks about the familiar concepts of ' function ' and ' purpose ' and points out that one sociological approach with which she will be concerned tends to interpret moral, political and religious activities and expressions in terms of their functions within a social order. But, she adds, politics in particular needs to be considered from the point of view of purpose as well as of function ; we must ask not only "What does this activity effect? " but also " What is it intended to effect? ". She then suggests that we need a further category to cover the capacity some people have to make individual contributions which are not adequately described in terms of social function or even of purposive action. With some misgiving, she chooses the general term 'powers' to cover both the individual flair and that sustained creativeness in certain kinds of role that she associates with 'vocation'.

In chapter II, Miss Emmet considers Societies and the two traditional methods of interpretation : that of the British social anthropologists and that of the continental sociologists such as Max Weber, Simmel and Pareto. She then turns her attention to some of R. M. MacIver's definitions (taken from his *Community : A Sociological Study*), and particularly to his definition of a society as "every willed relationship of man to man". This definition, she points out, compelled MacIver to write as though the primary fact about societies is that they are products of deliberate will and purpose. For her part, Miss Emmet suggests that when we talk of 'a society', we should simply mean "some form of empirical alignment which constitutes a relation between people in virtue of which we think of them as grouped". This, she thinks, will help us to realize that the notion of 'belonging' to a society may find both more and less sophisticated forms of expression, since the criteria for belonging will vary according to the kind of society involved. Later in the chapter, Miss Emmet makes a passing but interesting comment on the theory of the Social Contract. The truth behind this much criticized theory, she suggests, is the fact that social life depends on certain *expectations* as to how people will behave, and the fact that these are sufficiently often fulfilled for individuals to plan their own conduct accordingly. Some of these mutual expectations then become standardized in terms of socially recognized rights and obligations.

In this same chapter, Miss Emmet makes some useful observations about such terms as 'status', 'role' and 'social structure', discusses ably the distinction between *personae* and persons and comments favourably on the value of the 'field analogy' in social description. Whenever she can, she emphasizes the part played by the flair and enterprise of some individual, a part which, because personal and unique, will have to be omitted in a social analysis which "aims at generalized descriptions of role behaviour".

There is nothing sensational or startling in all this ; it is just plain good sense applied to the interpretation of many concepts which are the stock-in-trade of anthropologists and sociologists. And the same remarks apply to Miss Emmet's treatment of a question about which there has been much loose talk : the setting up of a World Government. She neatly dispels the illusion that this is something not too difficult to achieve, given a measure of goodwill all round :

> I have not much hope . . . of the feasibility of a 'World Government' in the sense of a legislative assembly of a federal kind for the whole world. For there to be a 'polity' (*i.e.* a society with government through commonly accepted political institutions) . . . there

> must be tacitly accepted norms as to how to behave, and intuitive understanding that there must be limits of opposition and intrigue. This probably depends on common traditions and habits, and would be difficult to achieve on an indefinitely large scale. There is also the administrative problem of making government effective on a world-wide scale (p. 39.)

Chapter III is entitled "The Notion of Function" and Miss Emmet starts off by considering some of the models of society used by social theorists to orient their descriptions. Undoubtedly one of the most dangerous of these, as most philosophy students are well aware, is the 'organism' model. Miss Emmet of course recognizes this and quickly passes on to another, which "looks on a society as a single system of interrelated elements with mutual adjustments and corrections, and examines the 'functions' of social institutions by trying to see how they contribute to maintaining this unity". There is surely some confusion here; for models, unless they are mannequins, do not examine anything. What she must mean is that the social theorist who adopts this particular model, *uses* it to examine the 'functions' of social institutions, etc. At all events, it is this type of non-purposive explanation which prompts Miss Emmet to give some close consideration to the terminology of 'function' and to develop the view that this concept has certain "teleological associations, if not implications". Attention is drawn to the fact that biologists have found the notion of 'function' to provide a way of talking about organisms as self-maintaining on-going systems in an environment, without postulating conscious purpose or design, and to the fact that this has suggested an analogy for a way of talking about social systems—talk not without its dangers. For it has encouraged people to speak of such systems as if they were more unitary and closely integrated than they in fact are. Thus, although it may be legitimate for biologists to use the term 'function' to get rid of the notion of purposive intention in the case of organisms, if this is carried over into social contexts, where purposes may very well be present, paradoxical results will ensue. While therefore we can understand the biologists' struggle against teleological language, we should not be too ready to assume "that the primary meaning of teleology should be explanation in terms of consciously entertained purpose". We should, Miss Emmet suggests, remember the Aristotelian roots of the notion as "the tendency of a process towards its *completion*, rather than the attainment of an envisaged aim".

To this, however, it may be rejoined that the biologists are at liberty to define 'function' in their own particular way, to suit their own programme of explanation, and that Miss Emmet is equally at liberty to inject her particular piece of teleology into the concept if she feels it necessary for *her* kind of social explanation. Like many other concepts used by philosophers and scientists, it benefits from precise definition before use.

Having discussed the notion of 'function' in mainly scientific contexts, Miss Emmet next proceeds, in chapter IV, to consider social customs and institutions in terms of their functions; and she is especially concerned with cases where the function, *i.e.* the role of a part in a system, is cited in relation to the system's maintenance in what is said to be equilibrium. This term 'equilibrium', first used in a sociological context by Pareto, and much favoured by social theorists, receives some sharp logical probing. Miss Emmet has little difficulty in showing that Pareto's use of the term, and equally that of Homans and of Chapple and Coon (in their *Principles of Social Anthropology*), all suffer, though in different ways, from grave defects—Chapple and Coon, for example, so use it as to make it applicable, though not by design, to any situation at all. She rightly points out that the concept of 'equilibrium' will only be fruitful in an empirical study if it is possible to distinguish the societies or groups which show equilibrium from those which do not. She suggests therefore that 'equilibrium' should only be used "where it is possible to show that customs, institutions and the social activities related to them dovetail in together in certain specified ways so that one provides a corrective to disruptive tendencies in another. It should also be possible to show how, if these functional relationships are lacking, a form of social life will break down; and also to show how a reacting tendency may go too far."

Chapter V, entitled "Purpose", is not easy to summarize, but Miss Emmet here deals appropriately with the logical misdemeanours of those who talk all too glibly of "a common purpose" or "the purpose of politics as such" or "the purpose of the State". What she says is merely common sense applied to political conceptions which, in the past, have often been endowed with a spurious mystical significance. Her next chapter—"'Open' and 'Closed' Morality"—covers a lot of well-worn ground, but she manages to criticize, with a light but effective touch, some of the familiar claims of Weber, Durkheim, Bergson, Lévy-Bruhl and likewise Professor Popper. At the same time she is scrupulously fair in acknowledging the value of the contributions made by all these and others to the clarification of this issue.

From this discussion of morals and religion in the context of 'open' and 'closed' societies, Miss Emmet turns, in chapter VII, to consider "Functions and Powers in Religious Symbols"; and it is here that she begins to work her way into the philosophy of religion. Her method of approach is to consider some interpretations of religion as (i) a stabilizing agency and (ii) "a source of powers making for new stages of development". Here, Miss Emmet seems more concerned with the various claims made on behalf of religious rituals as to their beneficent effects rather than with counter-claims as to their perniciousness, *e.g.* the part they play in creating or perpetuating irrationality, ignorance and misery; and I am sure that an examination of the counter-claims would have been equally if not more valuable. On the other hand, she makes some pertinent criticisms of the Freudian view that religion is to be explained in terms of a desire for comfort and security, since, as she rightly points out, religion usually causes its disciples to give themselves a whole host of new anxieties and obligations. The latter part of the chapter is devoted to prophetic symbols, nearly all chosen from the Christian religion, and to what Miss Emmet calls their 'promissory' character; and she is concerned to show that these symbols "both indicate and instigate a *process* already going on which we very imperfectly understand". But the illustrations which she uses to buttress this contention fail to do the trick and serve merely to throw light on the vicissitudes in meaning which various symbols undergo in the course of their long history.

The next chapter, entitled "Charismatic Power", is mainly concerned with the effects of blessing and cursing. It is somewhat tedious and is overburdened with Biblical interpretation—and quotation. There is however an interesting discussion, which should be of interest to the social anthropologist, of the many meanings attached to the word 'Mana'. A remark on the last page of this chapter paves the way to the next: "in considering the office of prophet, we have been approaching the notion of Vocation in its prototypic and most ancient form". In the following chapter, then, entitled "Vocation", Miss Emmet recurs to a topic which has continually pre-occupied her attention, namely, How can a social analysis, which moves on a level of abstraction which is not concerned with individuals, take account of individual creativeness? The brief answer seems to be that we must widen our conception of vocation, to take into account the person with creative talents or genial eccentricities, and that both sociology and social ethics must make room for this expanded conception. It is worth remarking that Miss Emmet nowhere makes clear what she means by the expression 'social ethics' except to say that it "should be concerned with matters of function and status"; but we must assume, from hints dropped in the definitions of 'vocational' and 'professional' ethics, that she envisages it as some sort of manual of normative social behaviour. This conclusion is reinforced when we come to her final chapter, entitled "Some Connections and Contrasts", in which she sums up some of her views and discusses "the main contribution which sociology is at present making to ethics". "A person", she continues, "may ask himself the ethical question 'What ought I to do?' and his answer will be deeply influenced by how he sees his structural relationships to the society about him." By this use of the term 'ethics', now without the prefix 'social', Miss Emmet tends to confuse her readers, particularly if they are not philosophers; for what she is really talking about is the aid which sociological findings can give to the individual faced with a moral decision. She is not talking about the value of sociology for ethics considered as a branch of philosophy which studies the logic of moral discourse.

My general impression of this book is that, while it may well be of interest to philosophers who are concerned to clarify social and political concepts, its main value, which is considerable, will be for anthropologists and sociologists. As regards philosophers of religion, I find it difficult to decide whether they will benefit from reading it.

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VIII.—NEW BOOKS

Personal Knowledge. By MICHAEL POLANYI. Routledge & Kegan Paul, 1958. Pp. 14 + 405. 42s.

THIS is a long and complex book, containing discussions of a great variety of philosophical matters. This review is concerned chiefly with the main theme of the book, which is the character of scientific thinking. Professor Polanyi attacks one theory about scientific thinking, which he calls Objectivism. This is the theory that scientific beliefs are completely determined by the observed facts, independently of the personalities or the choices of individual scientists; they can be verified by any observer, and thus they hold universally.

Against this view, Polanyi argues along the following lines:

(1) The propositions of science are not mere descriptions of actual and possible observations, and their truth or falsity is not entirely determined by observations. Even in physics, the most " objective " of the sciences, probability-statements are indispensable. But probability is not an observable fact; a probability-statement is a personal appraisal going beyond the observed facts.

(2) When a scientific theory is contradicted by an observation, it need not therefore be abandoned. When confidence in the theory is strong, the observation may be flatly denied, or coolly ignored, or explained away by some *ad hoc* hypothesis. Many actual instances are cited.

(3) Where there is conflict between two scientific theories (*e.g.* the Ptolemaic and the Copernican), or a scientific and an unscientific theory (*e.g.* the microbe theory and the witchcraft theory of the causes of diseases), or a theory with an admitted claim and one with a disputed claim to be scientific (*e.g.* Freud's psychology or Rhine's parapsychology), the conflict is not resolved by the discovery that one theory fits the facts and the other does not. All the facts can be made to fit either of the conflicting theories so long as they are interpreted in the light of the theory in question. To change one's theory requires, not merely an attention to the facts, but a transformation of one's whole framework of interpretation. In such critical decisions a scientist is largely influenced by the " intellectual beauty " of a theory as an earnest of its capacity to reveal a hidden reality.

(4) All learning of skills or devising of useful instruments and techniques involves an implicit commitment to certain assumptions about the nature of things, though of these assumptions we have either no awareness or only a " subsidiary ", not a " focal " awareness. There can be no guarantee of the correctness of such implicit assumptions; yet without making them we can gather no " facts ", no data for knowledge. For instance, in learning a language one acquires implicitly a set of assumptions about what kinds of things and processes there are in the world, and how they are to be classified. The acceptance of such an idiom of thought may shut one off from alternative ways of understanding the world. A member of the Azande tribe is incapable of formulating in his own idiom European views about the causes of disease.

(5) Knowledge can never be completely formalised. However precisely a principle is formulated, the formula still needs to be applied, and the application involves a personal expertise on the part of the interpreter which cannot be formalised. No formula can precisely determine its own

application; but every application affects the meaning and validity of the formula.

(6) Knowledge is always sustained by intellectual passion; propositions form no part of science unless somebody asserts them and commits himself to believing them. This passion has a part to play in determining what is and what is not " science ". For instance, we require a scientific theory to be interesting or important.

(7) Wholesale scepticism is barren. If one reasonably doubts p, it is on the basis of a belief in q. Scientists do not keep open minds on matters about which they are informed and concerned. If they did, they would make no discoveries.

For such reasons, Polanyi rejects Objectivism, and proposes to replace it by a different view of the character of scientific thinking. According to him, scientific knowledge, like all knowledge, comes into being by virtue of a *personal* commitment, motivated by intellectual passion. Scientific theories cannot be appraised, or even understood, except by those who have been taught a special skill. The exercise of this skill involves a commitment to certain assumptions about the nature of things, assumptions which are implicit in the scientist's intellectual operations, and which he must therefore employ without criticising them, indeed without being able to say just what they are. Scientific hypotheses are not validated by mere observations, but by observations interpreted according to certain preconceptions; and as this manner of interpretation is not logically compulsory, neither are the conclusions which are validated by its means. Scientific conclusions are convincing only to scientists, that is, to people who have been trained to think according to certain principles. They are believed by non-scientists, and by specialists in other sciences, on account of the prestige of scientists as a body, a prestige maintained by a set of social institutions, enthusiasms, pressures and persuasive techniques. These institutions might disappear, and science with them. Thus the acceptance of scientific truth involves a venture of faith. But this commitment, while " personal ", is not " subjective ". It may claim objectivity in so far as it is undertaken with a sense of responsibility, with a " universal intent ", in the hope that it will lead the inquirer to a better grasp of an objective reality assumed to be discoverable.

Polanyi's analysis brings out a number of features in scientific thinking which are often ignored, and clearly shows the inaccuracy and inadequacy of Objectivism as he formulates it. He is right in maintaining that scientific inquiry is not a receptive, slavish affair of noting, recording and sorting observations, but a venturesome activity of constructing symbolic schemes in the hope that they will illuminate a reality as yet incompletely known. He is right in insisting that whether a theory fits the facts is not obvious, but can only be decided by a skilled interpretative judgement, involving the risk of error.

If, however, Objectivism is not a correct description of the actual thinking of scientists, we can still regard it as an ideal to be pursued. We can maintain that the only *rational* ground for preferring one theory to another is that it fits the facts more extensively or more exactly. We can demand that all explicit theories and implicit assumptions (intellectual beauty or passionate commitment notwithstanding) shall be held subject to the requirement that they enable us to make testable predictions as to the character of our experiences. It is this requirement which renders the witchcraft theory of disease unacceptable, and Freudian psychology suspect. Polanyi seems in places to be maintaining that some beliefs held by scientists both are and should be immune from this requirement. I do not think that his arguments will establish this conclusion.

Granted that Objectivism is an incorrect account of scientific thinking, what harm is there in it? Professor Polanyi indicates two ways in which it does harm. Firstly, by implying a "mechanical conception of man", it leads to a dogmatically behaviouristic methodology which, while innocuous in physics and chemistry, becomes increasingly misleading as we pass from the study of organic processes, through that of animal behaviour, to psychology and sociology. Polanyi argues that if you wish to understand even an inorganic machine, you cannot do it merely in terms of physics and chemistry, to which the concept "machine" does not belong. Physics and chemistry can tell us the conditions of a machine's success and the causes of its failure; but to understand it as a machine, we must grasp its "operational principle", which involves a reference to the purposes it is meant to serve. (Thus technology is essentially different from science, though it makes use of scientific principles.) Similarly the understanding of a living organism requires an idea of right and wrong functioning, an appraisal of a performance. Physics and chemistry cannot describe organic activity as such; the completest possible knowledge of the positions and movements of physical particles would tell us hardly anything that we are interested in knowing. With the higher animals, and above all with man, our capacity to perceive the activity of the organism as a total activity is derived from our awareness of our own analogous activities. We do not merely observe the motions which our fellow-men go through; we apprehend their movements only subsidiarily, as elements in what is focally an apprehension of their minds (I suppose this means that the movements are taken as expressing conscious purposes). Here we must recognise a personal participation of the observer as a human being in the process of understanding. In observing other human beings, there is further a "convivial" situation in which the observed person is an active participant, helping to make himself understood. Thus any philosophy which tries to assimilate the scientist to a recording-machine must, if taken seriously, obstruct the growth of knowledge. This seems to me well worth saying, and well said.

In the second place, Polanyi holds that the objectivist ideal of knowledge, and its accompanying mechanical view of human nature, are socially and politically damaging. Objectivism insists on the world being regarded as a mechanical process, and discredits all kinds of faith in anything which cannot be exhibited to observation. Thus the only sort of moral passion it leaves room for is that kind which, like Marxism, disguises itself as an "objective" account of a determined course of events, and leads us into an unscrupulous totalitarian fanaticism. To meet this danger, the author hopes, by insisting on the inescapable fiduciary elements in scientific thinking, to encourage us to persist in other faiths admittedly indemonstrable, in particular those liberal ideals without which the survival of science itself is in peril.

Polanyi says some penetrating things here about the mutual reinforcement of historical theory and reformist passion in a doctrine like Marxism. But his main argument is not convincing. It is true that the modern fanaticisms are largely a response to the loss of religious and moral convictions. But, on the face of it, the man best equipped to resist their appeal should be one with strict standards of scientific proof, who keeps a sharp distinction (which Polanyi seems to disapprove of) between judgements of fact and judgements of value.

The nature of probability, truth, randomness, religious doctrine, learning, are among the other topics handled in this book in an interesting and stimulating manner.

There is a complex misprint on page 320, line 6.

C. H. WHITELEY

Theme For Reason. By JAMES WARD SMITH. Princeton University Press. (London: Oxford University Press), 1957. Pp. 209. 32s.

THE author describes his task as that of "clarifying the concept of responsible justification where standard models are inadequate". More precisely, he attempts to expose a basic fallacy in philosophy, particularly prevalent at the present time, and to outline a method which will avoid it. The fallacy is the 'dichotomizing tendency', namely the view that the two pigeonholes, *a priori* and deductive on the one hand, and empirical and inductive on the other, are sufficient accommodation for all propositions, arguments and procedures of justification.

The polemic against this preconception is on the whole successful and worthwhile, although it is rather odd that in this context Professor Smith, who rightly shows no reluctance to refer to recent books and articles, should not avail himself of the ammunition provided by Professor Wisdom and Dr. Waismann. Because this critique is carried out in somewhat general terms, it is not altogether clear whether the author's objection is to the two simple-minded pigeonholes of *a priori versus* empirical, or whether his attack is against pigeonholes as such—that is, whether he contends that all categorizing and classifying in philosophy involves a misrepresentation of the complexity of thought and talk. The latter thesis, which is more original and interesting than an attack on a naive dualism, is implicit in parts of the book, but unfortunately it is neither clearly stated nor debated.

The general attack on dualism is punctuated by various *aperçus* which cast much-needed doubt on some associated dogmas. One of these is the overworked analogy, which seems to arise from some ill-understood remarks by Wittgenstein, between the basic principles of a theory and the rules of a game. Smith points out that the analogy breaks down at a vital point: "In a game, the rules are laid down; you may play it or not as you choose; it makes no important difference one way or the other. In science what is laid down is a purpose—sometimes vague, sometimes relatively clear. We want to find out all we can about something; or we want to achieve control of a situation by accurate prediction. The purpose being laid down, the whole point of science is to develop a set of rules which will be dependable. Laying down the rules is not something which is done 'outside' the 'game'; it is not a *fait accompli* before we start to play" (p. 182).

Smith also makes a number of penetrating remarks about the notion of analysis in philosophy. He examines a short passage from Feigl's introduction to *Readings in Philosophical Analysis*, and shows that Feigl there uses the word 'analysis' in five quite distinct senses. The moral to be drawn from this, I think, is that if philosophy is to be properly self-critical, it must examine with special care its own linguistic-conceptual tools. In the light of the inductive-deductive disjunction, Smith is also enabled to make some sensible and judicious observations on Stevenson's theory of attitudes and the existentialist notion of 'commitment'.

Although he succeeds in demonstrating that black and white distinctions are misleading in philosophy, Smith's advice as to what colours are required by the philosopher's palette, and how he should distribute them, lacks clarity. This is in part due to the fact that he is attempting the enormous task of reconstructing philosophical method in too small a compass. There is also another reason for this, because Smith is attempting not only to remedy the deficiencies of philosophical dichotomizing, but also to provide a solution to a problem which he finds at the root of all theories in philosophy. The problem originates in Smith's conviction that all theories presuppose methodological and other commitments which they cannot themselves justify. " We must never delude ourselves by thinking that analysis can be 'mere'. There is no such thing as a piece of analysis which avoids committing itself, however far beneath the surface, on all the issues which are important. To analyse is to distribute emphasis. Every analytic movement is a *Weltanschauung* " (p. 13). These are fighting words, and Smith emphasizes them later. " . . . the defence of any piece of analysis ultimately the defence of a distribution of emphasis. There is no such thing as mere analysis which fails to distribute emphasis ; there is only analysis which refuses to be self-critical " (p. 172). This seems to be an exaggeration. There is no doubt that some analyses, *e.g.* phenomenalism, have involved a 'distribution of emphasis', a too vague phrase ; but it is arguable whether all have in fact done so. Would this apply, for instance, to Russell's definition of number? Why can't we, in philosophy, just get something *right* ? In any event, Smith's argument here seems to rest on an even more radical thesis, *viz.* that all analyses *must* involve an element of emphasis or interpretation. He does not show this, and it is difficult to see how it could be shown.

The problem which Smith attempts to solve arises in this way. Justification, he argues, is not the same as proof, whether deductive or inductive. Furthermore, every theory legislates as to what will count as a justification. There is, therefore, a fundamental circularity in this concept. " We must somehow justify our very fiats as to what we will (*sic*) accept as a justification " (p. 105). Smith does not wish to escape from this circle, for he does not regard it as a vicious one ; indeed, he attempts to extract from it positive doctrines in political theory and ethics. The chief political conflict of to-day, he maintains, is between the model of deduction, which finds the Kremlin and the Vatican at one, opposed by the John Deweys and Karl Poppers who plead for social engineering. (Hardly fair odds, one feels.) Both sides agree that some kind of proof is possible in political thinking. Smith will have none of this, and bangs both heads together. He contends that the basis of democratic political philosophy consists precisely in the rejection of the idea of justification by proof. For government by authority, whether it consists of a leader, a party, or social scientists, rests on the fallacy that proof is possible ; whereas " without the presumption of those who have thought that the validity of their own solutions can be demonstrated, you have no rational ground for ignoring what people want " (p. 117). This leads Smith to what he calls the 'principle of fallibilism', which he regards as fundamental in a rational approach to any topic. He states it in various ways, firstly as definitive of 'being reasonable' ; " a rational man realises that his own opinion may be wrong " (p. 117). And again : " It consists in recognition of the fact that you cannot prove that you alone are right " (p. 131). These two formulations are quite different, and each raises a difficulty for the notion of 'fallibilism'. To realize that one's own opinion may be wrong is not at all the same as to recognize that one's opinion cannot be proved right—still less, that no other opinion could be right, whatever that might mean. For we often correctly say that we are convinced about matters that are not susceptible of any proof; as I understand it, this is one of Smith's theses. Hence to admit the impossibility of proof is by no means to admit the legitimacy of contrary opinions. The other formulation raises this difficulty: to what extent does it make sense for someone to assert a belief and at the same time to say " . . . but I may be wrong "? The possibility may be raised as an entirely theoretical one, in which case it will convey no willingness on the speaker's part to countenance other views. If, on the other hand, the speaker raises the possibility of his being wrong as a genuine contingency, then his declaration of belief is thereby weakened—it becomes simply the announcement of an opinion, not a conviction. Now although Smith is clearly right in saying that in some sense it is a mark of a rational man not to insist on a monopoly of the truth, nevertheless it is essential to the claim that a belief is rationally held that no other opinion would be rational ('justifiable') on the evidence. There is an interesting and important problem here, and Smith deserves our thanks for having raised it, if only implicitly.

One final *caveat.* Although Smith often makes a polite bow in the direction of Kant, most references to that philosopher betray serious misunderstandings, in particular an allegation that Kant's conception of the noumenon is closely connected with Peirce's theory of truth.

To sum up: this is an honest and able book, which fails in the last analysis because it attempts too much.

Misprints, none of them important, occur on pages 67, 173, 185 and 196.

C. K. Grant

Aesthetics. Lectures and Essays by Edward Bullough. Edited with an Introduction by Elizabeth M. Wilkinson. Bowes & Bowes, 1957. Pp. 158 + xliii. 30s.

The Meaning and Purpose of Art or The Making of Life. Revised and enlarged second edition. By Arthur R. Howell. The Ditchling Press Ltd., 1957. Pp. 219 + xvii. 21s.

Aesthetics is surely one of the most difficult of all subjects to write about well; but that it can be done is shown conclusively by this selection from the Lectures and Essays of Edward Bullough. The lectures, which are here published for the first time, are those that he delivered in Cambridge in 1907; the essays, '" Psychical Distance " as a Factor in Art and an Aesthetic Principle' and 'Mind and Medium in Art' were published in 1912 and 1920 respectively. But compared with the more strictly philosophical writings of that period they date remarkably little.

Bullough, by profession a modern linguist, was also what Miss Wilkinson calls 'an ardent protagonist of the psychological approach to aesthetics' (p. xxvi); and his purpose in these lectures was to explain in these terms what he took to be the 'Modern Conception of Aesthetics'. There are, roughly speaking, four parts to his exposition. First, in order to show what Aesthetics is not, a discussion of certain standard objections to (*a*) the possibility and (*b*) the usefulness of Aesthetics as a systematic study. Secondly, a firm rejection of the traditional view that the primary concern of Aesthetics is with the nature of Beauty with a capital 'B', coupled with a vigorous statement of his own view that "Things of beauty are incomparable with each other *in point of beauty*" (p. 44). Thirdly, a more positive account of Aesthetics as 'the systematic study of aesthetic consciousness' (p. 69); which leads, fourthly, to an attempt (on fairly familiar lines) to distinguish the aesthetic consciousness as essentially contemplative and to contrast it with the 'practical', 'scientific' and 'ethical' attitudes.

This last point is taken up again and given a brilliant fresh formulation in the paper on 'Psychical Distance'. What Bullough meant by 'psychical distance' he explained by way of an illustration—an illustration which was, incidentally, quoted in full by Jane Harrison in her *Ancient Art and Ritual* as an introduction to the use she wished to make of Bullough's concept. "A fog at sea is for most people an experience of acute unpleasantness." Nevertheless, if one is able momentarily to abstract from the experience the practical dangers and unpleasantness and to attend exclusively to its immediate 'objective' features, it "may acquire, in its uncanny mingling of repose and terror, a flavour of such concentrated poignancy and delight as to contrast sharply with the blind and distempered anxiety of its other aspects. . . . It is a difference of outlook due to the insertion of Distance" (p. 94). And the essay as a whole is a first elaboration of some of the ways in which Distance may be said to be a vital factor for all Art and Aesthetics.

The final essay was the fourth of five contributions to a symposium; (like 'Psychical Distance', it too was published in the *British Journal of Psychology*, though the symposium was composed for a Congress of Philosophy at Oxford). It is in fact very much a symposium piece. A number of interesting points are made and briefly discussed, all of which bear in their separate ways some fair relation to the title of the symposium; but there is no unifying thread of argument. This is not at all to say that it does not make very good reading. But it is incomparably less striking a paper than that on 'Distance', which is one of those essays which, though one may remain uncertain as to how precisely they add up, nevertheless leave a permanent mark.

But what sort of a mark? Although the paper was originally offered to an audience of psychologists, it has not, so far as I know, led to any very notable developments in psychology. Indeed, it is not very easy to see just what the psychologist might do with the notion of 'psychical distance' in the form in which it is presented here. "Distance is obtained by separating the object and its appeal from one's own self, by putting it out of gear with practical needs and ends" (p. 96). But how, one is puzzled to know, could such a distance be measured in experimental terms? That some sort of comparative measurements must somehow be involved is clear, quite apart from the suggestions already strongly implicit in the very use of the term, from the explicit use that Bullough goes on to make of it. There is, for one important example, his principle of 'The Antimony of Distance', according to which "what is most desirable, both in appreciation and production, is the *utmost decrease of Distance without its disappearance*" (p. 100). Or again, his view that "there are two ways of losing Distance: either to 'under-distance' or to 'over-distance'". Neither of these theses are particulary hard to interpret in a general sort of way; both of them can be and are indeed used by Bullough himself, though with great brevity, to bring a good deal of illumination to problems of criticism—the problems, for instance, of the nature of comedy on the one hand and the nature of melodrama on the other. But *can* one sensibly ask as a precisely intended question just how close it is possible to get without losing distance altogether? Bullough maintained that "*theoretically* there is no limit to the decrease of distance"; but in terms of what sort of psychological theory can this be given a precise enough meaning?

In fact Bullough's main influence has been critical and literary, and his concept of 'psychical distance' has come to provide a new focus of critical appreciation, a new way of articulating points that had tended to become blurred by habit and misleading association. Whether it is in the last resort able to bear the systematic analysis and working out that Bullough himself would most certainly have welcomed, remains an open and uncertain question. But what is certain is that the question is worth taking up, and any philosopher interested in the general problems of the nature of evaluation and appreciation should welcome the stimulus of this book. I refer, moreover, quite deliberately to the *general* problems of evaluation. For it might perhaps turn out to be possible to make use of the concept of 'distance' to clarify the elusive distinction between favourable value-judgments and expressions of favourable feeling; and even, if 'psychical distance' really does emerge as a psychological concept, to subject the nature and basis of this distinction to empirical investigation. Speculative 'ifs', no doubt; but certainly well worth looking into.

The Meaning and Purpose of Art is the posthumous second edition of a book that was first published in 1945. It is, as one might indeed suppose from its full title, a very different sort of work. But it would be wrong to exaggerate these differences; and even more wrong to do what could be only too easy for the professional philosopher, namely to dismiss the book as a hopeless tangle and very little more. Howell does in fact share with Bullough at least two fundamental theses. He is insistent in the first place that the notion of 'art' is more restricted than that of 'the aesthetic'. Anything whatsoever may in appropriate conditions become an object of aesthetic perception, may be perceived, that is to say, in its own right with no interfering considerations—perceived, as Howell puts it, 'presentially'. "Works-of-art", however, "are the recorded experiences of the aesthetic perception"; "art is formulation" (pp. 17-18). And this leads in the second place to something very like Bullough's view that things of beauty are incomparable in point of beauty—as well as to one of this view's most characteristic difficulties. For if anything whatsoever may be perceived aesthetically, cabaret may give rise to as intense an aesthetic experience as a Beethoven quartet; but just as Bullough was led to talk of beauty of a higher or a lower order, so Howell has no wish to say that these experiences are of 'equal value'. He is aware of but uncertain how to resolve this difficulty. But it is at any rate an instructive difficulty to get into and one which anyone who reflects on aesthetics must almost certainly meet.

For the rest, his refrain is 'unity'. "A thesis of this book is that the fundamental desire activating a man's life is for *Aesthetic Unity* between himself and his environment" (p. 2). To feel aesthetically is to feel at home with oneself and one's surroundings; "intuitive unity is aesthetic unity" (p. 46). Moreover, unity between the perceiver and what is perceived appears to demand a (felt) unity *in* what is perceived. "Intuitive unity, as unified presence, is the beautiful. Intuitive disunity is the ugly. The attainment of intuitive unity is the object of the work-of-art. It is the beatific end to aesthetic aim" (p. 48). Unity and harmony are, of course, familiar aesthetic themes; but it is no clearer here than elsewhere why one should not feel a satisfaction at the 'presential perception' of disunity. An empirical generalisation about the way in which most people react? A value judgment? A definition? A bit of all three, most probably. But I have to confess that I find what Howell has to say by way of general theoretical elaboration of the nature of aesthetic perception cumbrous as well as repetitive and distinctly less enlightening than the distinguished critics quoted on the dust cover seem to have found it.

In my view, then, there is not very much in this wholly unacademic book for philosophers as such. But Howell is at his best, if the point can be put this way, when discussing neither aesthetics nor psychology, but art and the outlook of artists. Here, in concrete examples and above all in the account of the how and why of the development of contemporary art from Cézanne onwards, his profound love and knowledge of his subject really does carry illumination. It is better to be grateful for this, and for the many well chosen illustrations, than to concentrate on however justifiable grumbles about unhelpful generalities.

ALAN MONTEFIORE

Ethics and the Moral Life. By BERNARD MAYO. London: Macmillan & Co., 1958. Pp. viii + 238. 21s.

FOR Mayo, the crucial question in ethics is, How is it that a man's decision that he ought to do X can also decide him to do it? We shall make no headway in understanding this, Mayo believes, until we give centrality to the notions of 'control' and 'regulation'. Moral action is essentially action regulated and controlled by moral language. This regulation is achieved in two main ways: through evaluative expressions ('good', 'bad') and by imperatives ('ought', 'ought not'). Moral evaluations involve *standards* of preference, are not unrelated or quixotic as taste-judgments may be. Imperatives are supported not by standards but by rules and principles.

In his 'defence of relativism' in ethics, Mayo denies that only the absolutist can compare and assess rival moral codes without absurdity. Criticism of codes is perfectly legitimate, so long as I have a code of my own: but I need not claim absoluteness for it. It is only subjectivism that makes moral dispute impossible (subjectivism construed as the equating of right and wrong action with what one happens to like or not to like doing). Subjectivism is false because we do in fact discuss "reasons for and against moral decisions and moral judgments" and because there are in use criteria of acceptability in morals that are least analogous to truth- and falsity-criteria in the non-moral sphere.

Distinctively moral principles are universalisable in three 'dimensions': (i) "if the circumstances recur, I again ought to do the same act"; (ii) "anyone else in these same circumstances ought to do likewise". (Like Kant, Mayo thus asserts that universalisability is a necessary criterion of a moral rule; but, unlike him, he denies that it is sufficient.) (iii) The third dimension consists in the assumption of a person who makes a moral judgment that others too share his principles. Morals can claim 'rationality' in the important sense that whether we are playing the role of moral agent or moral critic, we apply in either case the same moral principles indifferently to our own and to other people's actions.

But what of moral disagreements and disputes about which principles to accept? Mayo does not see these as a threat to the rationality of morals: "there is no more need for rational beings to have a common morality than there is for them to have a common language : yet morality involves a common element of reason, as language involves a common element of intelligence."

Can moral judgments be true or false? Their 'truth' or 'falsity' must depend on that of the general principles from which they derive. But moral principles are not logically parallel to general descriptive statements : for the production of a counter-instance falsifies a general statement : but an important part of what we do in asserting a *moral* principle is to refuse to recognise anything as a counter-instance. We may conclude that the differences between the "criteria of correctness" of empirical statements and of moral judgments are slightly more impressive than the similarities. Truth-value had better not be claimed for moral assertions. But since there is a logical difference between truth-criteria and *meaning*-criteria, this does not entail their meaninglessness.

One of Mayo's most original and arresting suggestions is that dispute and disagreement are not peripheral but absolutely central to morality. Principles are, in fact, formed only when men are forced, through challenge and conflict, to reflect on their conduct and to formulate their policies. To assert a principle is to make or remake a *commitment*, to take sides.

But moral philosophy is not exhausted in the logical analysis of 'ought', 'good' and 'right'. To understand how moral judgments do and do not affect conduct we need an account of such concepts as 'self-control', 'conflict', 'conscience' and 'authority'. Mayo sensitively analyses the metaphors of control—mechanical and political metaphors—as they figure in the 'Reason-*versus*-the-Passions' problem. His own solution turns on the distinction between occurrences and dispositions. The intractable question, How can Reason control Passion? is transformed into the manageable question, How do people acquire "character-dispositions like patience, resoluteness and conscientiousness"? In each case, once the 'control' metaphor is sidestepped, there is an immediate gain in understanding.

The political analogy is not all dross, however. It properly sees that the concepts of 'authority' and of 'organisation' are fundamental to the elucidation of 'conscience'. By way of such elucidation, Mayo arrives at a theory that owes a good deal to Butler and more to Kant ('the kingdom of ends' and 'autonomy' are particularly influential). 'Authority' can be exercised and obeyed by one and the same person. (The Company Commander's order, 'Boots will be polished', can apply to himself as one of the Company.) The authority of *moral* rules lies in the moral agent's "readiness to obey a command, derived from the rule, and issued by himself to himself". The distinctive feature of moral authority is its unchallengeability. It is the authority by which one challenges all other authorities. The question, How does it happen that accepting a moral principle influences our conduct? now loses its mystery. There is no logical chasm to span between accepting the rule and regulating one's conduct. Nor is it necessary to personify 'parts of the soul', a commanding part and an obeying part, once the nature of self-directed imperatives is clearly seen. An ethic of rules and principles, Mayo claims, avoids the errors of over-intellectualised moral theories (moral judgments as statements of an odd sort of *fact*), and the errors of theories that cope only with the *practical* aspects of moral language (emotivisms, subjectivism). Moral principles are intellectual and dynamic at once.

But does not an ethic of universalisable principles violate the uniqueness and individuality both of moral agents and the situations in which they act? Not in any vicious sense : for to think morally at all entails the ignoring of uniquenesses, the comparing of one situation with another, person with person, claim with claim.

An ethic of duty and rule-obedience must be supplemented from the over-long neglected province of 'ideals' and 'virtues'. Virtue-words refer not only to tendencies in one's actions to abide by principles, but also to the kind of person whose actions they are. Mayo both recognises the recalcitrance of 'character', 'vice' and 'virtue' language to logical analysis, and the importance of rehabilitating them in serious discussion. They are essential to delineating what H. J. N. Horsburgh has called the 'aspirational' and 'inspirational' standards of morality, as contrasted with the 'self-respect' standard, the mere abiding by principles, the breaking of which incurs blame.

Three points of comment, in conclusion. (i) Mayo might have given some detailed attention to the arguments recently used by Miss Iris Murdoch (*Arist. Soc. Supp. Vol. XXX*) to the effect that the universalisability demand belongs not to morals *as such* but to the specifically liberal tradition of moral thinking. To assert that moral judgments must be universalisable begins to look suspiciously like a moral commitment itself, however true it also is that the demand has become built-in to our language. Should this be so, the range of generality of Mayo's analysis would be limited to this universalising tradition. But, of course, to restrict its application is by no means to *refute* it, within these limits. (ii) He tends to over-stress the differences between taste-judgments and moral judgments, and to lose sight of some relevant similarities. Paul Edwards brought out some of these very clearly in *The Logic of Moral Discourse.* (iii) Mayo criticises as 'circumlocutory' the utilitaran explanation of why courageous (etc.) action is to be praised. But the utilitarian can reply that his elimination of numerous allegedly fundamental value-judgments (*e.g.* 'courage is a virtue' *tout court*) more than compensates for this mild complexity in the analysis. The utilitarian *may* be unable to cope satisfactorily with virtues and vices : but this needs more demonstration than Mayo provides.

This is, however, a most valuable study. It is written throughout in a clear and economical style : it is scholarly, but at no point does it allow its theses to be swamped by allusion or polemic.

RONALD W. HEPBURN

Kant's Critique of Pure Reason. By T. D. WELDON. 2nd edn. Oxford : The Clarendon Press, 1958. Pp. xii + 331. 35s. net.

THIS second edition, like the first, falls into three parts. There is, firstly, a historical part with a useful account of some of Kant's less known German predecessors and contemporaries ; secondly, an analysis of the argument of the *Critique* ; and thirdly a discussion of inner sense and the transcendental synthesis. But Part II has been greatly enlarged, and Part III slightly enlarged, in the new edition. This makes the book much more attractive than it was originally.

Mr. Weldon's primary purpose was to explain 'how the Critical Philosophy is related to live philosophical issues'. In the excursions he makes with this end in view he is always acute, ingenious, interesting, and clear. Unfortunately, as he scans the surrounding landscape with a roving eye, he is apt to stumble rather too often on the slippery paths of Kantian exegesis. But his most startling defect is a failure to find the clue to some of Kant's central problems. He thus tends to construct fascinating solutions which are alien to Kant's thought and even flatly contradictory of it.

A short note like the present is not the place for an attempt to justify so summary a criticism, but I may give one brief illustration of what I mean.

Mr. Weldon is specially interested in inner sense, a subject that in all conscience is difficult enough in itself. On page 259 he excludes from inner sense all the data of outer sense in spite of the fact that according to Kant all the stuff or matter of inner sense—at least so far as it is matter for knowledge—comes from outer sense. Not content with this, he also excludes from inner sense all volitional and emotional states, though he had only to turn to the Fourth Paralogism (A357-8) to find that this is the reverse of Kant's doctrine. Naturally enough, he comes to the conclusion that—on these non-Kantian suppositions—'the problem of finding any content for inner sense seems insuperable.' Yet Kant himself makes it abundantly clear that the content of inner sense, on his view, is not only our feelings, desires, and thoughts, but also every idea whatsoever (including ideas of outer objects) considered—from one point of view—as a *state* of mind. To say with Mr. Weldon that inner sense is confined to bare *acts* of synthesis is to assert what Kant consistently denies—besides being, I should have thought, a doctrine indefensible in itself.

Any conjectural solution based on these wildly mistaken premises is unlikely to throw light on Kant's problems. Mr. Weldon's attempts to fill up a non-existent gap lead him to subtle speculations about the physiological problem of perception which are interesting enough in themselves; but in my opinion they have little or nothing to do with Kant's own doctrines, and can only make his view of inner sense more obscure than ever.

Mr. Weldon is, in short, an admirable companion for a philosophical conversation on topics which he may have been stimulated to think about by reading Kant, but he is no safe guide to the understanding of Kant himself. I find it hard to understand how so clever a man could succeed so often in missing the obvious: he may perhaps have been misled by an excess of ingenuity. But at least we can always see what he is getting at. It is the woolly thinkers who make it difficult for us to progress in the study of past philosophies or the creation of new ones, and wool was a commodity for which Mr. Weldon had no use.

I hope this criticism will not seem too blunt, but I believe that an honest expression of differences is what he would himself have desired. He once said to me, in his fruity, slightly ironic, voice: 'Good Heavens! I never expect any one to agree with me.' It is a sad loss to philosophy that we shall not hear that voice again.

H. J. PATON

Essays in Moral Philosophy. Edited by A. I. MELDEN. University of Washington Press, Seattle, 1958. Pp. xii + 216.

"IT is one of the peculiarities of the philosophic enterprise that it can, and unless care is exercised will, exhaust itself in philosophising about itself, its procedures and its goals to the neglect of those questions about which there can be no philosophy and no philosophizing at any level. This is not to say that talk about how to do philosophy is impertinent or unhelpful. There are times, particularly when our most strenuous efforts to resolve our perplexities fail in their purpose, when we do need to take stock of what it is we are about and how best to attempt to achieve our goals. But, except in such moments as these, the familiar talk about how to conduct philosophic talk might well be restricted to editorial asides or reminders." With the aim of getting down to some first-order discussion of problems in moral philosophy, Professor Melden has invited eight well-known philosophers to contribute to this volume essays on specific questions.

I can say at once that this aim has been fulfilled. There is a wholly welcome absence of talk about method ; instead we have a variety of methods ranging from the formal rigour we expect from Prior to the studied, and in its way no less rigorous, informality we expect from Ryle. To be sure, a great deal about method can be learnt from these essays by anyone who wants to learn it ; but he will learn, not by being told that the only proper method of philosophizing is such-and-such, but by being taken through examples of methodical philosophizing ; and this is as it should be. But I am disappointed that Melden did not see fit to encompass another aim, that of widening the scope of topics discussed. It seems to me not only that there are important topics that have been too much neglected in recent years, but that the topics that have not been neglected, important as they are, have suffered from over-discussion. When this happens, the counters that have become too familiar get worn so thin that their value is greatly reduced.

Now almost all of these essays are concerned directly with the twin concepts of obligation and condemnation and, though all the authors except Prior assume that to discuss these concepts is to discuss what it is to be under an obligation and what it is to condemn someone for something, few of them make a fresh start by examining the variety of expressions concerned and their actual role in discourse with examples. What moral philosophy needs is some fresh starts (some of which will doubtless turn out abortive) from unfamiliar angles, preferably dealing with peripheral topics. Ryle indeed does make such a start, since the concept of 'forgetting' is not one that immediately comes to the mind of a man about to embark on an inquiry in moral philosophy. But, true or false—and Ryle's essay seems to me to contain a number of statements of each kind—his oblique approach is more illuminating than more direct and familiar ones. Several of these essays are in fact contributions to current discussions. In view of the distinction of the authors it would be unnecessary and impertinent to add that, as such contributions, their merits are very high indeed.

One of the pleasures of reading this book is to turn from one essay to another. Prior's method necessarily involves—so this is no criticism—the idea that deontic concepts can be interdefined. That the trio 'obligatory', 'permissible' and 'forbidden' can be treated in this way is familiar ; Prior is concerned with a simplification of deontic logic according to which 'forbidden' (and, hence, the other two) can be defined in terms of the concept of a 'sanction'. This simplification, he says, is not just a dodge ; it involves the *discovery* that the logic, the pure structure of 'being perfect' is the same as that of 'escaping from what we fear'. Indeed he rests the claim of formalism as a valid method of doing moral philosophy on the aesthetic pleasure to be derived from the discovery of such parallels. Now, having greatly enjoyed Prior's essay, it would be churlish in me to suggest that such aesthetic pleasure is an unworthy end ; but I am not convinced that the parallel is a beautiful discovery, since it does not seem to me to be a discovery at all. How could one show that the pure structure of two different concepts was identical or parallel ? Leaving aside, as merely captious, the criticism that no formal scheme can adequately represent the actual concepts of discourse, one would have to start with the construction of separate calculi for the two concepts, in this case 'being perfect' and 'escaping from what we fear', and then show that the two calculi were formally parallel or that one could be included in the other. Such a discovery would be exciting indeed. But Prior's conclusion is reached in a quite different way. It depends on getting us to accept as plausible the following assumptions: (i) that the familiar trio, 'obligatory', 'permissible' and 'forbidden' can all be defined in terms of a 'sanction', that is to say something which we fear, and (ii) that 'being perfect' is the same as 'fulfilling all one's obligations'. The first assumption is built into the system at the start, indeed it is just in this that the 'simplification' consists; and the second is needed to persuade us that the calculus of escaping can also be applied to 'being perfect'; so the parallelism is not in fact *discovered* at all. As so often, the philosophical questions are begged at the start.

It seems almost by design that the contributions of Hart and Urmson should be largely concerned with the examination and rejection of just these two assumptions. Urmson shows that 'being perfect' can by no means be identified with 'fulfilling all one's obligations' and it is a commentary on the present state of moral philosophy that this should need saying. Similarly Hart is concerned to confine the words 'duty' and 'obligation' within reasonable bounds and to explore their connection with threats and penalties. That there is some connection goes without saying; but even though the word 'obliged' provides us with an easy bridge between 'being under an obligation' and 'being compelled by threat', we can hardly take these two to be identical. Hart starts his discussion of the idea of 'a duty' by remarking that we not only have duties; we can also create, impose, incur, or assume them and that they can be varied, modified or extinguished. These are "some of the remarkable things that can, in any developed legal system, be done about duties, and hence be said about them". So the moral philosopher should start with the question 'How are the creation, imposition, etc. of obligations possible?' To this Hart answers: "This Kantian-sounding question can only be answered by describing in detail how in fact they are done." There can be little doubt that such detailed descriptions must at least precede, even if they do not provide the answers.

Hart's topic is also closely related to that of Frankena who is concerned primarily with the question whether one can have an obligation and not recognize it and the question whether one can recognize or admit that one has an obligation without having any tendency at all to fulfil it. Does the moral 'ought' of obligation logically imply the motivational 'ought'? In spite of the great subtlety of Frankena's argument—he certainly shows that I, among others, have been far too cavalier on this issue—I remain unrepentant in my belief that *moral* obligation, in one sense, of that phrase, does entail motive. But this only shows, I think, how useless, for the purposes of moral philosophy, the word 'moral' has now become. There is certainly also a sense of the phrase 'X is under an obligation' in which X can be under an obligation without recognizing it and in which he can recognize it without having any tendency to fulfil it. This is clearly the case with legal obligation, and the answers to Frankena's questions really turn on the extent to which we are prepared to assimilate moral to legal obligation. Hart shows that even legal obligation cannot be understood without, among other things, noting the fact that the words and deeds of properly qualified persons are accepted as constituting a standard of behaviour; and this seems to me also true of moral obligation. Without such an external source of law and its *de facto* recognition, we cannot speak of moral obligation at all; but obligation is not co-extensive with morality. The idea that it is may be partly responsible for the dispute with which Frankena deals.

P. H. NOWELL-SMITH

David Hume. By A. H. BASSON. Penguin Books, 1958. Pp. 183. 3s. 6d.

AN introduction to Hume is valuable, for while the *Treatise* may be a philosopher's delight, it is not a philosophy-beginner's delight; what to a tutor is admirably lucid and wonderfully ingenious may to his pupils be a longwinded and tortuous stretch of eighteenth-century opacity. If we are surprised that it fell still born from the press, we have no business to be. To reduce his book to Pelican length Mr. Basson has deliberately confined his attention to the three main subjects of epistemology, morals, and religion. And within these he has imposed further restrictions, excluding in particular Hume's treatment of space and time, and of the particular virtues and vices. Thus the claim made on the cover of the book that this is " a simple introduction " has a *prima facie* plausibility. Whether in other respects it is simple may be more doubtful: Hume's difficult account of belief, for example, is not made easier to understand when it is as compressed as Basson has had to compress it, especially if it is to be handled critically as well as expositively. And the student who *needs* a simple introduction to Hume may not make much of the interesting but not unpuzzling suggestion that some of Hume's statements about perceptions are definitions intended to provide a framework of analysis (*e.g.* that perceptions are either impressions or ideas), but that others are empirical statements (*e.g.* that a perception must appear to be what it is, and be what it appears).

Hume's purpose is stated to be the refutation of the ancient and false belief that as well as the realm of empirical knowledge (*that* things are so) there is the higher grade realm of metaphysical knowledge (*why* things are so). For Hume we can know *that*, and we can know *how*, we cannot know *why*. Not because we are not clever enough, but because our understanding is limited to what we can imagine, our imagination to what we can experience, and our experience to *thats* and *hows*. Hume's method in turn is stated to be that of getting the reader to acknowledge for himself that the tasks required for the attainment of metaphysical knowledge are beyond his capacity—the method aptly called by Mr. MacNabb in his book on Hume (which might have been accorded a mention somewhere in *this* book) the method of challenge.

Two chapters on the imagination and the understanding take the reader through the outlines of Humeian epistemology, with particular and justified emphasis on the status of empirical beliefs: there is no method of arriving at propositions about the future which guarantees their truth. The question is not what we ought to believe, or would be justified in believing (as though we had some freedom of choice in the matter), but what we do believe, and what makes us believe it. We believe as we do as the result of custom and experience; how could we do otherwise?

Basson interprets Hume not as *analysing* the idea of cause in terms of the imaginative idea of necessary connection, but as explaining how we (mistakenly) project this idea into our experience of sequences. This is of a piece with Basson's general view of Hume as a sceptic, and a total sceptic at that, denying knowledge of matters of logic as well as of matters of fact. It is essential to Basson's view that he should have shown that Hume denies knowledge of matters of fact. All that he has shown is that Hume denies knowledge of matters of fact *of some kind, viz.* the kind involving causal inferences or expectations.

The chapter on morals (with a tailpiece on religion) is not satisfactory. He starts with the role of reason in conduct, then drops it without comment in favour of talking about the is-ought passage, his treatment of which is liable to mislead the reader who looks to him as guide. The first part of his discussion suggests that Hume is criticising a view which finds *some connection or other* between factual and moral questions, so that the way to answer the second would be to answer the first. The second part of his discussion makes it clear that Hume was talking only about the view which held moral propositions to be *deducible* from factual propositions. But, as Basson does not revert to the earlier discussion, the reader might well suppose that according to him Hume regarded factual and moral propositions as unrelated—which he certainly did not. Hume on the moral sense is very sketchily dealt with, and will no doubt result in as plentiful a crop of misinformed essays and examination answers as have been produced by Professor Broad's remarks on the same subject in *Five Types of Ethical Theory*.

In the next chapter, The Nature of Things, Basson returns to what he is more interested in, and gives a much more meaty exposition of, and comments on, Hume's two questions: how are perceptions related to material objects? and how are perceptions related to the minds which have them? In the final chapter he rounds off his theme of Hume the sceptic—a sceptic in the sense used by Sextus Empiricus: one who cannot come to conclusions about occult matters, but " acquiesces in the sentiments which are the necessary products of immediate experience ". Here again one must regret the brevity forced upon the author, and hope that he has not written his last word on Hume.

A. D. WOOZLEY

A Modern Introduction to Logic. By J. W. BLYTH. Boston: Houghton Mifflin Co., 1957. Pp. xvi + 426. $5.50.

AMERICAN textbooks of formal logic sometimes devote an introductory chapter to placing the subject in the context of a general theory of signs. They tell us that this theory, semiotic, has three parts, semantics, pragmatics and syntactics, and that formal logic belongs to syntactics. The present book is distinctive in not being content with a mere sketch-map of this territory; six chapters out of nineteen are concerned directly with pragmatics or semantics. Under the heading of pragmatics five different functions of language are distinguished: the informative, the practical, the expressive, the ceremonial and the logical; as part of semantics we have detailed discussions of four main modes of meaning: denotation, signification, connotation and expressiveness; there is also a chapter on definition. These topics are important; they certainly belong to a logic not too narrowly conceived and they are interestingly presented in Professor Blyth's book. Professor Blyth may however be criticized for a careless use of language which suggests that his distinctions have a wider application than he intends or than would be justified. He makes statements about words in general which are true at best only of certain classes of words, namely nouns and adjectives. If any reader should doubt this let him study pages 115 to 117 and then try to classify, *e.g.* the word ' the '. Indeed the whole of semantics as treated in this book appears to apply solely to nouns and adjectives, including nominal and adjectival phrases.

The part of the book dealing with formal logic proper begins with an informal but extensive account of class logic including immediate inferences and syllogisms. Propositional logic is dealt with next and finally there is a chapter on strict implication. In this part of the book the author makes a great deal of use of matrices, not only, in the usual way, when explaining truth functions but also in his accounts of class logic and of modal logic. One advantage of this treatment is that he is able to bring out clearly the existential assumptions underlying the modern logic of classes and Aristotelian logic respectively. He is also concerned to stress the analogies and also the differences between class logic and propositional logic but here perhaps his treatment is less successful. Throughout this section on formal logic the dominant stress is on the application of logic to everyday arguments. In general we have here a treatment of formal logic which is slightly aloof from prevailing tendencies and for that reason, among others, worthy of attention. However it does not seem to point beyond itself in any obvious way; for example there is no mention of multiple quantification ; again although there are hints of more abstract developments of the subject there is nothing which could really be regarded as an introduction to any of these developments.

The last three chapters of the book deal with probability and scientific method. An interesting feature of the account of scientific method is the author's use of an ingeniously constructed artificial example to illustrate his points. Professor Blyth believes that artificiality is necessary for the sake of simplicity, *i.e.* presumably, to avoid complications irrelevant to the main principles which are being illustrated, but he tells us that his example is analogous to a real instance of scientific method, the development of the periodic table of elements.

J. A. Faris

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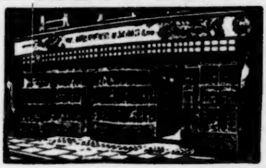

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